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AN ADDRESS

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By **Henry Gibbons, Jr., M.D.**, President, San Francisco, Cal.

Ladies and Gentlemen of the Medical Society of the State of California: Forty years—not centuries—look down upon us. But forty years have elapsed since the first session of the Medical Society of the State of California, succeeding its organization, yet they have been years of most brilliant and unexampled achievement in the domain of science and art. As we count time this is scarcely a generation, though reckoned by the rate of progress a hundred years ago it is not *one* century, but centuries. So great has been this progress that notwithstanding we are closely linked with the early beginning of our Society, its story is almost ancient history.

I need not recount in detail the lines upon which this great advance has been made. It is almost as "household words." And yet reflection will surprise us that so much has been accomplished in our time.

So vast has been, and is, the diffusion of knowledge, so rapid the interchange of thought from man to man and from country to country, that no important fact in science, no new methods or inventions remain the exclusive property of individuals or peoples for even a brief period. No sooner is a discovery made than "the world knows it by heart." In fact, so eager is the profession for the brand new, that it often seizes the premature and unperfected results of investigation, not infrequently to have the idol marred or dashed to pieces, as in the case of Koch's tuberculin.

In this connection I offer a tribute to the profession of this, geographically, far away corner of the earth, for its alertness and enterprise. It will not be left behind in the race. Nothing better emphasizes its capacity and its cosmopolitan character than the readiness with which it acquires a knowledge of and applies in practice the widespread discoveries in the world of medicine.

The literature of medicine has become almost discouraging in its immensity. It is quite impossible for one mind to cover more than a moiety of its range. Sixteen royal quarto volumes, aggregating over 17,000 pages, are required to give the subjects and titles only of the articles in pamphlets, magazines, and books in the library of the Surgeon-General's office at Washington. Four hundred new books on medicine and surgery, and their allied subjects, are issued every year. At least 130 medical magazines, daily, weekly, and monthly, are published in the United States alone. So extended has become the field of study that specialism is a necessity, and that which forty years ago was condemned as a phase of quackery is now not only accepted but invited. Each of the principal branches of the healing art—medicine and surgery—has been enormously expanded until each has subdivisions more elaborate and extensive than its parents forty years ago. The anatomy of the nervous system, with reference to localization of diseases of the brain and cord, microscopical anatomy, abdominal surgery, gynecology, diseases of of the eye, ear, nose, and throat, and bacteriology, are ample evidence of this. Pathology is a new science since Virchow, while physiology, chemistry, pharmacy, and even obstetrics, are so vastly augmented and improved that they would scarcely be recognized by the founders of our Society. Instruments and methods of precision in examination of every part of the body, its individual tissues, secretions, and excretions, have multiplied far beyond anticipation. Surgery has received an inconceivable impetus from the extraordinary discovery and development of antiseptic methods; and medicine seems upon the threshold of similar progress in consequence of investigations in serum-therapy. Thanks to coal-tar, the chemists and the commercial spirit, our list of drugs is now almost limitless. Scarcely a day passes that the mail does not bring to our offices some new candidate for professional favor accompanied with a mass of literature upon every subject from acne to zoster, and affording a liberal education in therapeutics. Out of this chaos we have rescued, however, a goodly array of most valuable remedies that vie in excellence with the few noted ones our predecessors have transmitted to us.

A physician of forty years ago placed in a modern hospital would find himself quite unequal to the varied duties. Our established theories of diseases and their causes would be largely unknown to him. Our methods of examination would be in the main new and untried. The uses of the various specula and mirrors would be a sealed book; bacteriological investigations and the elaborate examination of urine, blood, and milk would present unlearned problems; he would seek upon the shelves in vain for many familiar drugs only to find them replaced by those of which he had never heard; and if, perchance, he escaped a shock from witnessing brain, abdominal, and pelvic operations, he could not fail to be appalled at the elaborate *technique* of antiseptic surgery.

But while we congratulate ourselves upon the brilliant achievements of the present, and feel thankful that we live in so progressive an age, I have no desire to belittle our forefathers in medicine. "There were giants in those days." With the facilities at hand they accomplished much. Our great progress is made possible by the preparatory work done by them and by the wonderful advance made throughout the entire field of scientific research.

I may be pardoned for recalling the fact that Dr. Henry Gibbons, Sr., presided over the deliberations of this Society forty years ago. This circumstance it is that has led me into a reminiscent mood. Organized in 1856, when the State was not yet six years old, its first session thereafter was held in Sacramento, February 11th, 1857, with members and delegates numbering fifty-six—a goodly showing for such a young and sparsely settled State, of magnificent distances and slow and primitive means of communication. Were the attendance upon our session of to-day proportioned to the number of physicians at present in the State, this hall could scarce contain them all. There still remain with us eleven of these early pioneers; the others have gone to their long rest. The Transactions of this early session is a small pamphlet of 43 pages, containing the President's address and eight papers. Nine standing committees presented but three reports, two of which were exceedingly brief. Our present Transactions have expanded into a volume of 350 to 450 pages, the nine committees into twenty-one, nearly all of which provide extended reports.

It is interesting, in this connection, to study the question of the duration of professional life, and yet with the data at hand it seems impossible to arrive at a very definite result. The second City Directory, published in San Francisco, in 1859, when a population of 78,000 was claimed, gives the names of 157 physicians, of whom only eight are now left in practice in the State. The State and City Directories of 1867—thirty years ago—name 764 physicians, of whom 104 are still living in the State; but from 30 to 50 of these have retired, leaving only 60 or 70 in practice.

Of those registered in our official Register of physicians, up to March 2, 1877, 835 in number, considerably more than half have either died or left the State. Three hundred and eighty only remain. In the following ten years 1,241 additional registrations were made. Nearly half of these last registered have died or moved away. Thus out of over 2,000 physicians registered in the first half of the last 20 years about one-half only remain. In the last ten years nearly 2,500 physicians have received licenses, and of these at least 700 have died or left the State; therefore, in a total registration of about 4,500 physicians in 21 years, there yet remain with us over 2,800. But the departures are constantly exceeded by the arrivals. For the first ten years of the registration period an average of about 100 yearly were licensed; for the last ten years this average had risen to 250; quite out of proportion to the increase of population. Record is obtained of the death of about 50 physicians annually; a considerably larger number either die, retire from practice, or leave the State, so that at the present time in the neighborhood of 150 physicians are each year removed in some way from practice, leaving a yearly excess of about 100.

Whence come all the physicians who are each year licensed? They come from all parts of the earth. California seems to be the Mecca for physicians; with this difference, however, that after they make their pilgrimage here, they remain. There are now in California about 400 graduates of colleges of foreign countries; 1,650 graduates from colleges of the United States other than California, and 750 from California. Of the 270 physicians licensed the last year, 26 were from foreign colleges, 144 from colleges of the United States other than California, and 100 from California.

There has been an astonishing increase in the number of medical students at all the medical centers, both in this country and in Europe, in the last few years. In the United States in ten years 21 new colleges have been established, and the number of students has increased from 10,000 to 17,000; this in the face of a constantly appreciating standard for graduation. All over the country has this increase of standard taken place. Entrance qualifications or examinations, lengthened courses, and an increased number of years of study are now universally required in the United States, and yet in ten years the number of students has grown 66 per cent.

No doubt the causes for this are the same elsewhere as here, but here they have acted with peculiar force. It is unquestionable that an important factor is the impetus in a literary and scientific direction, resulting from the founding of a great University on this side of the bay, which has turned many minds in the direction of medicine and kindred pursuits that otherwise would have chosen different walks of life. Contrary to what was expected, this new University did not draw prospective students from Berkeley, but a phenomenal increase of students at both universities took place, so that in the case of the latter, at least, means were proposed to limit the number of applicants. Thus, notwithstanding the two medical colleges of this city have been in advance of the large majority of the colleges of the country, in this that they earlier instituted an educational requirement, and added to this requirement for entrance; doubled the number of years of attendance; nearly tripled the aggregate number of months of lectures, and exacted more frequent and elaborate examinations, the number of students has continually augmented, and far beyond expectation.

A year or more ago it was supposed that the matriculates would diminish in number in consequence of the depressed financial and industrial condition of the country, but the reverse taking place, the curious theory is advanced that this is the very cause of the present increase; that young men having been crowded out of the ordinary avenues of business and seeking other means of livelihood, have chosen a professional career. It must be borne in mind that they have not been enticed into the colleges, for, as I have shown, higher standards for entrance, of study, and for graduation have repeatedly been exacted. About one-third only of the yearly accessions to the profession are from our State institutions.

The hope may be father to the belief that the time is coming when some requirement other than a diploma from a college shall entitle to practice. Former presidents have discoursed upon this subject, and at former sessions it has been considered by the Society, but only by constant agitation can we expect results. While California may be said to have inaugurated the recent advance in the profession in its legal and educational aspects, for to her belongs the credit of enacting the first medical law, she has now fallen behind in the race. Nearly all the States have since enacted more restrictive laws, notably that recently passed in New York.

There is no reason why we should not have not only a licensing board, but an examining board, before which all future applicants for registration shall pass examination prior to being admitted to practice. It must be done for self-preservation. Then the various colleges being relieved of the onerous and often disagreeable duty of examining would be enabled to devote themselves to the strictly educational; and emulation in this direction would be legitimate, commendable, and to the best interests of the student, the profession, and the community. This is the goal to which we are tending. It may be yet a long way off, but we will reach it the sooner if we keep it constantly in view. This Society and its members can do much by constant agitation and action, and can take encouragement for the future by that which the past two decades have wrought.

But I would not stop here. There should be a National Board, as long ago proposed, and I would spell it with a big "N," for we need not fear centralization in this regard. There should be a national law regulating the practice of medicine; a National Board of Health—its president to have a seat in the cabinet—to have general supervision of all matters pertaining to the public health. A license or diploma from such a board, or from sub-boards stationed at important centers, would have more than even national value; it would give professional standing to its possessor the world over.

And how important the other duties that would fall to the lot of a National Board of Health! Surely epidemics, endemics, diseases of cattle, and other of the lower animals; farm, dairy, and other food products, the adulteration of foods, the influence of climate in health and in disease, and many other matters pertaining to preventive medicine, are of sufficient national and international importance to demand a more general management and supervision than can be given by State and local authorities. Surely a department of health is of as much importance to a community as a department of agriculture. Vanderbilt found several separate roads, under as many managements, dividing the distance from New York to Buffalo. By uniting them under one supervision he increased their efficiency a hundred fold.

Climatology has always received much attention at the hands of the profession, but the practical side of the question is yet imperfectly appreciated. Too little is known of the relative value of localities as regards climate and disease; and what is known is not sufficiently available for ready use. The instructor in climatology at Harvard—Mr. Robert C. Ward—is mooting the project of establishing a course of instruction upon this subject in all the medical colleges. He urges the injustice of advising change of climate unless the influence of such climate upon the disease be well understood; that it is essential to the medical man to know the effect of weather conditions upon the various pathogenic bacteria, and why changes of season modify the occurrence, the development and the propagation of disease; and that he should be provided with all meteorological data, together with an accurate knowledge of the physiological effects of the various climatic elements upon the human body.

It is only necessary to observe the lamentable want of system in the use of the various mineral springs of California to appreciate the justice of these observations. It is true that the physician is not often responsible. The ordinary health-seeker finds it sufficient to go to some "springs" upon his own responsibility; he does not consider very deeply the medicinal qualities of the waters, and to drink as much and as often as possible is the sum of his appreciation of their value. All this irregularity is largely incident to the development of a new country, but the wonderful wealth of California in this direction makes it all the more necessary that the subject should be properly understood.

A communication from the Medical Society of Bernalillo county, New Mexico, which will be read by the Secretary, asks the coöperation of all medical societies in urging that the Weather Bureau republish the tables in its report for 1885, incorporating with them the very elaborate data accumulated since that time, together with some account of the topography, geological formation, natural drainage, nature and source of the water supply, vicinity of mineral springs, etc., at each station where observations were taken. This would seem to be a legitimate matter for the consideration of the Society.

Sanitation and preventive science in our day have grown to enormous proportions. We see it in the purification of the water supply, the inspection of milk, meat, and food products; the plumbing of houses, the disposal of sewage, the better provision for the sick and destitute and defective, in homes, asylums, schools, and prisons, in the care and feeding of infants, in the prevention of epidemics, and in a thousand and one other directions. Boards of health and health officers have multiplied over the land, until almost every town is supplied. Eighty different localities within the confines of California have their health officers or boards, or both, which represents an immense amount of gratuitous work by the profession.

Is there a parallel to it in other professions or callings? Does the public appreciate the untiring and self-sacrificing labor of our profession toward the prevention of the very source of its income and livelihood; that it is through these labors that the mortality rate has fallen to half its former figures, and that even in a decade the rate in such cities as Berlin, New York, and New Orleans has declined from 20 to 30 per cent.?

The great epidemics that formerly ravaged Europe and America are losing their terrors for us; thanks to a better knowledge of their character and to an efficient quarantine. Small-pox, yellow fever, and cholera are not the dread diseases that they once were, and the plague of India is apparently held mainly within the limits of its origin. The bacillus of cholera has long been known. Its discovery led to proof of the mode of propagation of this disease. The welcome intelligence now comes of the discovery of the bacillus of yellow fever, of the plague, and of the rinderpest. Soon more efficient measures will be applied against these dread diseases, and thus the work goes bravely on.

Among the good works inaugurated by our local Board of Health is the crusade against the adulteration of food. The extent to which the latter is carried is almost incredible. Scarce any article of prepared food is exempt, so keen is competition, so reckless the greed of money getting. No thought of the deleterious influence of this sophistication upon health and life seems to deter the manufacturer. The number of deaths of infants directly due to impure milk must of necessity be large. How serious the effect of adulterated foods upon those of larger growth cannot be readily estimated. The increased power granted to the present, over former boards, is a matter for congratulation to the profession and to the community. We should strengthen their hands in the good work.

While there can be no question as to the benefits to be derived from measures looking to the prevention of food adulteration, some opposition, both here and elsewhere, has arisen in consequence of action taken regarding some of the contagious diseases. Restrictive measures in this direction have been a gradual growth. In earlier days some of the profession and the laity were disposed to rebel against quarantine of small-pox. Now no one, at least in theory, opposes such quarantine. Then scarlatina and diphtheria were put under the ban, with little opposition. But now tuberculosis, typhoid fever, and measles are added to the list. Personally I question this action in regard to measles. It seems to me ill-advised, and without definite object, or if with object, then for present purpose only, and without thought for the future. The intention I take it is to prevent the spread of the disease. Even if this can be done, is it wise to do it?

Measles, in many respects, is radically different from all other important contagious diseases. It is the most readily communicated, and hence the least readily prevented from spreading. It is the least serious both as regards immediate results and sequelæ. It has no known means of prevention, as small-pox. It is not protected against by age as scarlet fever or diphtheria. If stamped out in a large city it would be practically impossible to quarantine against it. The rapid and extensive interchange of members of communities would preclude such purpose. It is less severe in childhood than in adult life, and its untoward effects are less readily avoided in the latter.

During the recent war regiments of soldiers born in isolated regions of Maine and the Western States, which the disease had never reached, were attacked with it, and numbers died. Into the Faröe Islands, which the disease had not visited for 65 years, it was introduced in 1846, when old and young alike were attacked, and the resulting distress was of exceptional nature since, nearly all lying sick at a time, systematic nursing was impossible. How desperate would be our strait in this city were measles, after a possible exclusion for a term of years, to be introduced. The distress, the mortality, the private and public pecuniary loss would be incalculable. Properly treated, and not over-treated, the mortality from measles is extremely small; among the well-to-do it is almost nothing. Far better would it be to instruct in its hygiene which seems so little understood by the laity—the physician often not being called—than to attempt its suppression.

With regard to tuberculosis the case is somewhat different. At the outset one feels repugnance at the thought of making public record of a physical condition and resultant mental distress made known to him in his professional capacity, and without the accomplishment of any good result, so far as the patient is concerned. First and last there is room for a good deal of difference of opinion upon the propriety of a measure which at least has the appearance of being premature. It is well enough to prevent promiscuous spitting, to educate the public to the understanding that tuberculosis is mildly contagious, and that it may often be cured in its early stages, and to adopt any reasonable measures to prevent its spread, but to require public record of all cases incipient or advanced, will serve to invite concealment, and seems to some extent a reflection upon the capacity of the physician.

The fruition of the hope that some measurably satisfactory cure for tuberculosis might be discovered upon the lines suggested by Koch, or in some other direction, has been many times deferred. Of the various plans hitherto proposed, none has stood the test. How great the boon to humanity would be the discovery of a remedy for this mightiest of all destroyers no previous experience can suggest. Indeed, were its dread progress stayed, it seems doubtful if the Malthusian doctrine might not demand more support. The latest candidate for favor is the discovery of a resident of our own State and city, who, after many months of research and experiment, has arrived at results which appear startling and irresistible, and if corroborated by more extensive tests and experience, will mark a never-to-be-forgotten era in medicine.

You will, to-morrow evening, have an opportunity of judging for yourselves as to the value of this discovery, which, though immediately concerning tuberculosis, involves a principle applicable to all the diseases of bacterial origin. If, as now appears, the cure of a large proportion of cases of moderately developed tuberculosis can be assured, the action of boards of health in requiring the early report of cases would be of vast importance, since it would place them in line of immediate curative treatment. Meanwhile, the principle of serum-therapy seems established as regards diphtheria, and is gaining ground in other directions, as with hydrophobia, tetanus, cholera, pneumonia, and typhoid fever. Thyroid extracts are accomplishing some remarkable results, and the use of gland extracts in general is being pushed with the possibility of revolutionizing some methods of treatment.

The wonderful discovery of Roentgen, alluded to by our President of a year ago, has already established itself as an invaluable aid in diagnosis. The discovery itself is not more wonderful than the rapidity with which practical use is made of it. Though so far removed from the general medical and scientific centers, San Francisco has, from the first, been well supplied with experimenters and apparatus. Practical demonstrations of the value of the X-rays in medicine and in surgery will be offered at the present session.

The question of the medical fee for life insurance examination, which received attention at the meeting a year ago, was happily solved by one of the companies by a return to the original fee. The subject of life insurance examination might claim more attention than has heretofore been given it by the Society. There are many points imperfectly appreciated by medical examiners, and since they are of great importance and magnitude to the companies, to the insured, and to examiners, it would be well to bring them more clearly to the attention of physicians. Among our members are several who are specially interested. Let them enlighten us at some future meeting.

One who has much riding to do about a large city must hail with intense satisfaction the substitution of asphalt pavement for stone. Not only is the roughness of riding diminished, but the noise is lessened most agreeably. Some one has said that noise is one of the penalties of civilization, but the amount of noise produced seems quite out of proportion to that which is necessary. The rattling of wagons over the streets, the jingle and rumbling of street cars with the clanging of their gongs, the clashing and whirr of machinery, the blowing of whistles, the ringing of bells, the earthquake-like shaking of houses by passing fire-engines, immense butcher wagons, and monster trucks, the cries of peddlers advertising their wares, and sounds from many other sources make up an aggregate of noise that a bedlam might envy. Noise has become a nuisance. We cannot escape it—

> "For the intruding sound would enter,
> Heedless of ceremonial or decorum,
> Den, office, parlor, study and sanctorum"

Fortunately the influence of habit saves us much of the distress that strangers in a great city suffer, but even then the nervous system cannot be so constantly taxed without more or less permanent result. If the nerves of the strong and well be made to suffer, how much more must those of the weak and sickly. It is certainly not merely an assumption that considerable of the wear and tear of life comes from this incessant stimulation of the nerves of hearing. Is there a remedy? The replacement of stone by asphalt, as already stated, is one. In some European cities, I am told, the playing of pianos after 10 o'clock, is prohibited by law. A study of this subject from the physician's standpoint might furnish data to justify municipal or legislative action.

In this inconsequential sketch of mine, while reference has been made to some features of medical progress, it has not been my office nor my intention to even outline what the year has brought forth. This duty devolves upon the various committees, which are the machinery of the Society, and that the duty has been well and faithfully performed, the programme before us will testify. A word, however, in regard to the appointment of these committees. It has been no insignificant task. With a strong desire to be impartial, to consider the various interests, to recognize the claims of those who had not yet served the Society, and to place members on such committees as are in line with their spheres or tendencies in practice, I may still have failed of complete success. This has arisen largely from the impossibility of knowing in which direction a considerable proportion of the members can do the best work, whether in practice, in surgery, or in some special branch. I would, therefore, suggest, as an aid to future presiding officers, that a record be kept by the Secretary, showing the direction or directions of interest or practice of each individual member. A is a surgeon, a gynecologist, an oculist, or an aurist; his preference is well known; but B is a general practitioner who may be specially interested in surgery, or obstetrics, or pediatrics, or diseases of the nervous system, or dermatology, or in two or more of these branches.

Finally, let me call your attention to the fact that our programme is the longest that has ever been presented to the Society; that it includes forty-three papers, discussions upon which will be opened by thirteen members appointed for that purpose, besides reports from eight committees and officers, and demonstrations and discussions, occupying two entire evenings. This will necessitate the greatest expedition in the transaction of business, and the most rigid economy of time, and will require strict adherence to the twenty minute rule in regard to papers, and the five minute limit in debate.

I congratulate the Society upon what it will accomplish to-day, and upon what it has already accomplished; upon the contributions it has made to medical literature; upon the liberal education it has afforded its members, the friendships it has formed, the fraternal feeling it has fostered; upon the influence it has exerted in and out of the profession, and upon the still greater power for good which is in store for it, when it shall include within its folds a larger proportion of the medical men of our State.

REPORT OF THE COMMITTEE ON CLINICAL MEDICINE.

Of the Medical Society of the State of California, April, 1897.

By JOHN FIFE, M.D., Chairman, Red Bluff, Cal.

The Diagnosis and Treatment of Typhoid Fever.

The wisdom of selecting for the chairmanship of so important a committee one who, though he burn much midnight oil, burns it not in his study, to assist in illumining his mind, but beneath his buggy, to light his road, may well be questioned; and the wisdom of its acceptance by such an one is still more questionable. The truth is that when I accepted this appointment I was less wise than willing, and that must serve as my apology for appearing before you to-day. I have chosen as the subject of this report "The Diagnosis and Treatment of Typhoid Fever," both because of its importance and of the new light which is being shed upon it.

It goes without saying that the value of statistics relative to methods of treatment depends upon the correctness of the diagnosis, and it is equally certain that this is frequently a very difficult matter. It can rarely be made with certainty during the first week; it frequently cannot be made until late in the second week; it sometimes is not made at all, and, not infrequently, it is made when it does not exist. Much stress has been laid upon the prodromal symptoms and the slow onset as an aid to diagnosis, particularly to distinguish it from fevers of malarial origin; but, though of value, I believe their importance has been much exaggerated. The gradually ascending temperature, of which we have heard so much, is spoken of by Wünderlich as follows: "The course of the fever is typical and perfectly characteristic; it distinguishes abdominal typhus from any other disease. Besides, the cases which exactly follow the regular type, there are others which deviate from it—irregular cases."

At the time of my graduation I was particularly impressed with the first sentence of this quotation, and had completely overlooked the qualifying sentence, so much so that, upon being asked by my preceptor for the diagnostic symptoms of typhoid, I merely pointed to my thermometer. He smiled; and, though I understood, I failed to appreciate its value until after I met with my first case, which was of the irregular type; and I may as well say here that they have been of that type pretty regularly ever since. The temperature was as high at my first visit as at any time subsequently. I made a diagnosis of remittent fever, and did not correct it until the commencement of a series of intestinal hemorrhages.

Enlargement of the spleen, iliac tenderness, and gurgling, and tympanites, singly, are not of great importance; the eruption does not appear before the eighth or tenth day, and is absent in about one-third of the cases. In short, while the totality of symptoms enables one to make a diagnosis with reasonable certainty early in the second week, in a majority of cases, in a large percentage it is attended with much uncertainty; and any method which promises greater accuracy ought to be welcomed by the profession.

The method of Widal, published in June of last year, promises this much. Indeed, at one time it was thought that it would prove to be an infallible test. I shall not enter into a detailed description of the method, as it is familiar to all, but will briefly state its salient points. As originally published, it was recommended that a trace of a virulent culture of the bacilli be placed upon a slide, mixed with a drop of distilled water and a drop of the blood, recent or dried, of the suspected typhoid patient, added to it. If it were typhoid, the bacilli, in from 5 to 30 minutes, "clumped," or became agglutinated and lost their motility.

These phenomena had been previously noted by Pfeiffer, but to Widal is due the credit of pointing out their diagnostic importance. Numerous observers have confirmed these statements; but they have also brought to light the fact that the same results occasionally obtain when, apparently, normal blood is used. These results also usually follow when the subjects have previously had typhoid; and they are sometimes seen in diabetes, influenza, and perhaps in other diseases. It has also been found that the reaction is sometimes caused by the blood of birds and of the horse, rabbit, and other animals. Further experimentation, however, has apparently shown that when an attenuated culture of the bacilli, largely diluted, is used, the true reaction occurs and the pseudo-reactions fail.

Much yet remains to be done before the value of the method can be exactly defined; but enough is known to justify the expression of the hope that we are about to come into possession of a method which will relieve us of a great quantity of statistical lumber relative to new methods of treatment, having for its basis diagnoses drawn from fever, diarrhea, and iliac tenderness and gurgling.

In the long interval between the announcement by Currie, that typhoid might be cut short if his method of applying water externally were used, and that of Lee, of Chicago, that the fever in his cases usually ceased in a week by the *internal* use of water, there have been many advocates of "abortive methods" of treatment of this disease. It is true, as stated by the elder Flint, many years ago, that "it is not an unreasonable expectation that an antidote, or parasiticide, as effective in typhus and typhoid fever as quinine in malarial fever may hereafter be discovered;" but it is, unfortunately, quite as certain now as then that we must look to the hereafter for its fulfillment,

The statements of certain enthusiasts that "death is a wholly unnecessary consequence of typhoid if properly treated early," and that the occurrence of intestinal hemorrhage in the practice of a physician should be made a criminal offense, punishable by imprisonment in jail, is of "such stuff as dreams are made on." The Woodbridge treatment is still in evidence. While we are not, perhaps, in a position to pass final judgment upon it, we may at least say that some of its claims are highly extravagant; and we are now hearing something of the other side of the story.

In a discussion before the New York Academy of Medicine, Dr. Francis Delafield stated that he had used Woodbridge's treatment during the past winter in 30 cases, following it literally at first, and afterwards omitting everything but the calomel and guaiacol. He observed no difference between the effects of the original and the modified plan. The epidemic was a mild one, and of the 30 cases, he lost four. He concluded by saying that, although in the mild cases, the duration of the disease might have been shortened a little; in the severe ones there had been no influence, and that it was not at all probable that the drugs would at all diminish the mortality of the disease.

Dr. Charles E. Nammack, in an article in the *Medical Record*, states that upon assuming the full service in one of the medical divisions of Bellevue Hospital, he placed all of the new cases of typhoid upon the Woodbridge treatment, with instructions to the house staff to follow the details of the originator implicitly, except that patients with a temperature exceeding 104° F. were not to be denied the benefit of the tub-bath. He states that "a study of the charts did not show that the usual course of typhoid fever was in any way modified by the treatment;" and that he "sincerely regrets that his hope that the treatment of the poor man's typhoid fever had been found is still deferred."

Dr. Woodbridge was visiting New York at about this time, and was given exclusive charge of four patients in the wards of Bellevue. In his reply to Dr. Nammack's article, he found fault with the use of the tub-bath while his treatment was being used, and thought, besides, that Dr. Nammack was not sufficiently familiar with the treatment. He also gave the histories of the 4 cases treated by himself. It is perhaps worthy of note that one of them died upon the eighth day of the treatment.

In 1861 E. Brand published the first article upon the method of treatment which has since been known by his name. A considerable improvement in it has been made by continuous, but gentle rubbing while in the bath. In the limited number of cases in my experience, in which I have been able to carry out this method systematically, I have found it to possess all of the merit claimed for it by its advocates; and I believe it to be by far the best treatment now known. Unfortunately, it can be carried out, in the country at least, in but a small percentage of cases, and we are compelled to resort to other measures.

Many years ago, Wünderlich advocated the use of calomel in typhoid, claiming that it would sometimes cut it short, and that in the majority of cases, when given during the first week, the course of the disease is rendered milder and shorter. His statements have been confirmed by Niemeyer and others. I believe that it has merit. In a considerable number of cases in which I have used it, I have thought that it mitigated the severity of all of the symptoms, but I have yet to see a case cut short by it.

My usual plan is to give 10 grains at a dose, repeating it every 24 hours, until three doses have been given. I also give an occasional large dose of quinine for its antipyretic effect, and employ frequent sponging with cold water and alcohol, otherwise meeting indications as they arise. Under this, as under every other known plan, patients will die occasionally, but the mortality is small.

The success which has attended experiments in the artificial immunization of susceptible animals, by means of inoculation of sterilized cultures of the bacilli, encourages us to hope on, if we hope ever, that the reasonable expectation of Flint is to be realized, and that it is to come through the new field of therapy.

REPORT OF THE COMMITTEE ON SURGERY

Of the Medical Society of the State of California, April, 1897.

By T. W. HUNTINGTON, B.A., M.D., Sacramento, Cal.

Surgical Progress in the Last Decade.

It happens that ten years ago, at the annual meeting of this Society, I had the honor and the pleasure of reading the surgical address. Upon that occasion you were incidentally reminded of the generous impulse that had been given to the surgeon's art during the recent past, and there were suggested the almost limitless possibilities which flowed naturally from the teachings of the most beneficent law-giver surgical science has ever known. To-day I follow a custom from which, on public occasions, I have rarely deviated, and again pay humble tribute to Sir Joseph Lister; and it is with pleasure that I quote the following from the last number of the *British Medical Journal:* "Long after the present generation of surgeons are gone, men will see for themselves what he was like who made a new thing of surgery. There is Listerism, but no Hunterism or Cooperism. What Lister has done is what we all do. All can raise the flower, now all have got the seed."

He had disclosed the principle that was for all time to govern wound treatment. Listerism, or asepticism, if you prefer the later nomenclature, had become the universal shibboleth of surgeons. A correct interpretation of scientific truth had elevated surgery to the dignity of an exact science, and it only remained for the industry, intelligence, and boldness of a later epoch to formulate methods and perfect *technique.*

The logical order of human progress was being observed. Surgical science had confessed its dependence upon natural law. The true relation of cause to effect was finally established, and the hitherto unattainable was placed within easy grasp. A recent writer has shown by an ingenious argument, that the subjugation of electricity means for thought and work, not an addition, but a multiplier, and in this permutative principle of the multiplication of factors he finds an answer to the query, why progress is by leaps.

The last half century affords two admirable illustrations of this general truth. The discovery of anesthesia, in 1846, dispelled, as by magic, the dread of human suffering, dissipated the terrors of the surgeon's knife, and removed from operative procedures the stigma of barbarism. The natural sequence was a wonderful activity in the surgical world, and a general advance in surgical methods.

But while anesthesia broadened the horizon a thousandfold, ideal surgical excellence continued to be the dream of the enthusiast until asepticism became an established fact. By its agency the permutative principle was again applied, and the field of action was once more multiplied. You will recall the vicissitudes through which it passed, and the long period during which it was held by many as another of the disappointing visions of the visionary. But asepticism had come to stay.

This, then, was the situation in 1887. Enthusiasm was at its highest pitch; methods of research were revised; new issues were joined; old theories were abandoned, and there sprang into existence an abiding faith in systems founded upon rational hypotheses. No longer was wound infection, that Nemesis of former periods, an universal *odium chirurgorum*, to stand as an insuperable barrier to surgical advance, and far less often was the blighting humiliation of defeat to paralyze the arm uplifted for the alleviation of human suffering.

Here began an era of experimentation. The authority of the experimenter asserted itself in no uncertain tones, and his prerogative was beyond peradventure. Already were new campaigns in contemplation. Organs which previously had been placarded as forbidden ground, were recognized as amenable to successful approach, and conditions that were traditionally hopeless were being subjected to careful analysis under the search-light of modern science.

About this time an eminently respectable authority announced with perfect seriousness that the art of operative surgery had attained the highest possible standard of excellence, and that in future we need hope for little of originality in this department. These words echo in a remarkable manner the dictum of the illustrious Baron Boyer, who, at the close of the Napoleonic campaigns, is said to have proclaimed that "surgery seems to have attained the highest degree of perfection of which it is capable." Is it not particularly significant that within a decade after such a declaration every important organ of the human system has been subjected to surgical treatment by original methods, and, for the first time, successfully in more than one instance?

A review of the history of a period so eventful, so replete with achievements, so notable for general progress, must be, of necessity, cursory and in a large measure unsatisfactory. No apology will be made for omissions of descriptive details or statistical inaccuracies. Your attention will be centered upon the main features that characterize this epoch, and signalize it, surgically, as a golden era.

First, let us note some of the latter-day revisions of surgical methods and *technique*. Perfectly sterilized catgut as a substitute for silk in the capacity of buried sutures and ligatures has given remarkable assurance in the rapidity and permanence of wound closure. The frequency of ligature abscesses and persistent sinuses in cicatricial areas, caused by the presence of unabsorbable silk, provoked a careful search for a material less objectionable. While silk may retain some adherents in the management of pedicles and in some departments of intestinal work, until catgut is superseded by something more nearly ideal, it will maintain itself as the best adapted to all ordinary surgical purposes.

The matter of closure of abdominal wounds seems to be satisfactorily settled. The plan of uniting each of the various layers by the tier method very nearly reëstablishes normal relations, and ensures a firm cicatrix. This step will rarely admit of hernial protrusions as an unfortunate sequel to abdominal incisions. The former method of through and through sutures involving peritoneum and superincumbent tissues, *en masse*, is unsurgical, and, in the light of recent investigation, unsafe.

The third and last item under this head has reference to a very efficient and almost invaluable resource in contending with shock and loss of blood. It has been found that sterilized normal salt solution injected either into the veins or into the cellular tissue beneath the skin, exercises a potent influence in sustaining cardiac action during periods of profound exhaustion. The subcutaneous procedure will be found more readily available than venous injections, from the fact that when such a step becomes necessary the veins are nearly or quite empty, and are picked up with more or less difficulty. Furthermore, the loss of time necessary for venous injection will cut an important figure, and will often determine the attendant to adopt the subcutaneous plan.

The amount of fluid which will be readily appropriated is quite remarkable. In a recent case of double amputation I found it to the interest of the patient, who was well nigh exsanguinated, to inject 3½ quarts of fluid during a period of 1½ hours, the time occupied by the amputation. I believe this means will often carry patients through an operation that would otherwise succumb to the conditions present.

No region of medical science has been more persistently or laboriously studied of late years than that dealing with the brain and the nervous system; and if brain surgery cannot show the striking successes that have been achieved in other fields, it is neither for lack of work nor solid acquisitions of knowledge. In this field surgical interest has specially centered round certain questions which it will be well, briefly, to review.

The great advances made in cerebral localization gave ground for hope that brain tumors might be more successfully excised, an expectation that has been only partially realized. It has to be admitted that localization cannot yet be effected with desirable precision. Thus, out of a list of 162 operations reported last year by M. Allen Starr (*Medical Record*, vol. xlix), no tumor was found in 48 cases. Nevertheless, 72 recoveries was no mean justification for the work done.

Cerebral abscesses have absorbed much attention; and the consenus of opinion is towards exploratory aspiration and free drainage. Though few recover, relief is generally afforded, and without operation such cases are necessarily fatal.

Epilepsy has perhaps been the center of interest in brain surgery, and out of a cloud of reports and opinions something like agreement is gradually being evolved. Thus it is coming to be pretty generally admitted that general non-traumatic epilepsy, while occasionally benefited by operation, as it often is by any change of treatment, is not properly in the surgical field; that focal epilepsy, whether traumatic in origin, or associated with infantile cerebral palsies, may be properly operated on within a reasonable time after its development; that the chances of success decrease as this period is prolonged, until after two years it will be very small indeed. Opinions still differ as to whether such operations should be limited to the relief of pressure, or extended to free excision of the probably diseased cortex. Sachs recently reported 19 cases, whereof 3 were positively cured, 2 greatly improved, 3 somewhat, and 11 not benefited.

The operation of craniectomy—that is, section or resection of a portion of the microcephalic skull, for the relief of idiocy, has shown but few successes. It is now generally acknowledged that the small skull is the result and not the cause of the small brain. Yet the few successes recorded would seem to justify the operation, since, whether fatal or successful, the result will alike benefit the patient and society.

Operations for the relief of trigeminal neuralgia by excision of the gasserian ganglion have rapidly multiplied, and are justifying themselves by their results. The operation was first performed by Rose, of London, in 1890 who, attacking the base of the skull by way of the face, removed the ganglion by a trephine opening at the foramen ovale. He was followed by Horseley, who opened the skull in the temporal region, cut through the dura, raised the brain, and excised the ganglion. It resulted fatally, and the operation has not been repeated. In the following year Frank Hartley, of New York, taking advantage of the fact that the ganglion lies between two layers of the dura mater, followed the intracranial route indicated by Horseley, but did not interfere with the dura until the ganglion was reached, and then only divided its coats, leaving the upper surface, bounding the brain, intact. The same idea was developed by Krause, of Altona; and this operation, now known as the "Hartley-Krause," is at present the one most generally followed. Both the operations of Rose and the Hartley-Krause have been repeatedly performed, and with about equal success. Thus Keen, in 1894, gave a list of 38 cases, 19 by each method, wherein both scored 17 successes. Nevertheless, the greater facility by which the Hartley-Krause method can be performed, has given it the preference amongst recent operators. Last October Murphy, of Chicago, collected 57 cases, with 5 deaths. In this operation, as in all operations on the skull, the general tendency has been to make use of the method of opening by bone flaps.

The vermiform appendix seems to have attracted more attention, surgically, during the past decade, than any other individual organ. Prior to 1887 it had been recognized as a probable factor in the conditions known as typhlitis and perityphlitis. In 1885, Frederick Treves wrote elaborately upon the surgical anatomy of the appendix vermiformis. In October, 1886, R. H. Fitz, of Boston, clearly established the importance of this organ in its relation to pathological conditions. To this latter communication, more than any other, is due the wide-spread clinical researches which succeeded it. Professor Fitz's observations are recognized as classical, and are more frequently referred to by authorities than are those of any other writer.

In 1887, Dr. R. F. Weir, in a paper published in the *Medical Record* of June 11, treated the subject of laparotomy for perforation of the appendix vermiformis exhaustively. Weir, at that time, was committed to the policy of dealing with the appendix, only when perforated and attended by formation of pus. About this time there arose a discussion regarding the propriety of anticipating the later and more dire conditions attending this disease, by appendectomy.

The claim for priority for the operation of the removal of the appendix during the quiescent period, seems to rest between H. B. Sands, of New York, and Frederick Treves, of London. I have been unable, after a painstaking search, to locate the date of Prof. Sands' first operation along these lines. Treves reported his first case of interval operation in 1888, and another in 1889. J. Greig Smith claims the honor for Prof. Treves, but in doing so, gives credit to Morton, of Philadelphia, as having first suggested the rationality of this step. Treves' first case was classed as one of operation for diseased appendix after an attack of perityphlitis. In 1889, the term appendicitis had been coined, and Treves accepted it as established in surgical nomenclature. Chas. McBurney's first interval operation was done on May 20, 1888, and Nicolas Senn reported two cases in 1889. W. T. Bull, in 1888, read an elaborate monograph advocating early operations for perityphlitic abscesses, but neither in his New York, nor in his later London address, did he make any allusion to appendicitis as a distinct pathological condition, nor to operations during the quiescent period.

You are familiar with the spirited and oftentimes acrimonious discussion which was elicited throughout the civilized world by this procedure. For a time the consensus of opinion seemed to favor the idea that the risks attending abdominal exploration were so grave as to render interval operations unwarrantable. It is worthy of note that Frederick S. Dennis, of New York, in 1890, in an article published in the *Medical News*, protested strongly against the tendency to interval operations, which had become very frequent. He was, however, immediately traversed by Treves and numerous other authorities who had early become converts to the legitimacy of the operation. The bolder and perhaps wiser operators, have remained steadfast to their conviction that appendicitis is a distinctly and positively surgical condition, amenable in a large proportion of cases to surgical treatment, and to surgical treatment only. My own first interval operation was done August 20, 1891, and I have resorted to the step with tolerable frequency since that time.

My views upon this subject were fully expressed in a communication read before the California Northern District Medical Society, in October, 1895, and published in the OCCIDENTAL MEDICAL TIMES, January, 1896. The following is a quotation which I have taken the liberty to make from that paper: After the recital of a case, I said: "I am by no means prepared to say that the life of the subject of this disease (chronic recurrent appendicitis), was in immediate danger. Nor do I wish to debate the question whether or not skilful medical treatment might have averted the operation entirely, after a protracted period of invalidism. I do, however, maintain, and I wish emphatically to urge this point, that such a patient's prospects for a quick and safe return to health were through the medium of an operation done in an improved manner during the quiescent period. Furthermore, the physician or surgeon who temporizes with such a case for sentimental or other reasons, and adopts the so-called medical plan, assumes a responsibility that may eventuate in calamity to the patient and humiliation to himself.''

This I believe to be the sentiment most widely accepted throughout the surgical world to-day, and if we are to be governed in our future action by the disasters of the past, and the more fortunate results attending later and safer procedures, it would seem that the rule for our guidance would call for prompt operative interference.

The history of the radical cure of hernia is of particular interest. From the earliest periods to the present time persistent effort and wonderful ingenuity are to be noted in perfecting a method for the safe and reliable correction of the hernial condition. No positive success was attained, however, until a high standard in wound treatment had been established, and asepticism had measurably eliminated wound infection as the chief danger in this as in all surgical procedures. Then we find records of operations done by excision of the sac and suture of the pillars. But the percentage of failures was so great as to throw discredit upon the plan, and it was not until 1888, when Bassini publicly announced the method which has made his name familiar to us all, that the term ''radical cure of hernia,'' was anything more than a misnomer.

Halstead, almost or quite simultaneously with Bassini, announced the method which bears his name, and which differs from that of Bassini only in unimportant details It must be accepted as a fact that Halstead's operation was devised independently of Bassini, and the credit of originality will be shared equally by these two eminent authorities.

The condition upon which must depend the legitimacy of this procedure, has been very clearly stated by Coley in the *American Journal of Medical Sciences*, July, 1895, as follows: ''The mortality must be either *nil* or not greater than that associated with the condition prior to operation.'' Bassini, in 1890, published 251 cases with 1 death, and Marcy, in his very exhaustive work, refers to over 3,000 cases which were attended by a mortality of less than 1 per cent. Coley's statistics show one death in 360 cases. Many other series substantiating these figures might be referred to, but for the purposes of this paper it is unnecessary.

The second condition bears upon the probability of success, either as regards cure or relief of the deformity. As technical details have been perfected, there has been a noted and rapid diminution in the number of relapses, until there seems to be no reasonable objection to the step from this standpoint. My own cases, over 200 in number, in which various methods have been adopted, show but two relapses. Coley reports 300 cases of Bassini's operation with kangaroo tendon sutures, with two relapses.

While Bassini's operation has been very generally accepted, it is but proper to mention the work done in this department by other authorities, notably Macewen, of Glasgow, and Marcy, of Boston. To W. B. Coley is due the credit of establishing the fact that operative treatment is both safe and reliable in its application to the herniæ of children. A. E. Rockey, of Portland, Oregon, has recently done a most valuable service in connection with the herniæ of the aged. In an article published in the *New York Medical Journal*, February 20, 1897, he has collected from various sources, 61 operations done upon persons over 60 years of age, with but two deaths, or a mortality of but 3.5 per cent. Rockey's conclusions will doubtless meet with general approval, and the operation will be recommended to the aged in suitable cases, as a matter of increased safety and comfort.

Such an enormous amount of work has been done in the field of intestinal surgery in recent years, that it is futile to attempt more than an allusion to it by generalizations. I know of no grave structural lesion of the digestive tract that has not been attacked surgically, and in a vast majority of these conditions with highly gratifying results. Gastrotomy, gastrostomy, gastro-enterostomy, pylorectomy, enterectomy, intestinal anastomosis, colotomy, and excision of the rectum, are terms that are becoming more and more familiar and acceptable.

Many of the very recent additions to our knowledge are in the line of surgical treatment of gastric and intestinal perforations, due to ulceration. An article by John M. T. Finney, of Baltimore, in the *Annals of Surgery*, March, 1897, is of unusual interest, as bearing upon the results obtained in the treatment of perforating typhoid ulcer. He states the general proposition as follows: "Without operation the chance of recovery is very small. Now and then a case has recovered after what appeared to be a perforation of the bowel. With operation the chances are a little better than one in four, or 27.65 per cent."

Clearly in this class of cases the consideration is not what is the percentage of mortality of the operation? but what is the percentage of recoveries after the operation? According to Finney there have been in all 52 cases, with 17 recoveries, or 32.68 per cent., a truly remarkable showing. The surgical treatment of gastric perforations, due to ulceration, is even more satisfactory than typhoid perforations, provided that the operation be done within a brief period after the occurrence of the accident.

The matter of intestinal anastomosis has attracted widespread attention, and there can be no doubt that in future one or another of the many methods advocated by various authorities will be resorted to with increasing frequency. Since the announcement by J. B. Murphy, in 1892, of his discovery of the ingenious contrivance known as the "Murphy Button," the plan of procedure has been greatly simplified; and while the appliance does not meet the approval of all authorities, it is a fact that resort is had to it more frequently than to any other method.

A large number of cases of enterectomy for gangrene of the bowel, caused by strangulated hernia, have been recently reported. M. H. Richardson's suggestion in this connection is clearly rational—that where, during an operation for strangulation, the condition of the bowel is in doubt, the wound should be left open, the bowel carefully packed with iodoform gauze, and left *in situ* for 12 to 36 hours, when its integrity may be definitely determined, and measures for the removal of the gangrenous portion be adopted when occasion requires.

The procedure known as Loreta's operation, seems for some reason to have fallen into disuse. Very few cases of this sort have been reported during the past five years. I have recently operated upon three cases of poloric stenosis due to cicatricial contraction, two by dilatation according to the method described by Loreta, and one by pylorectomy and gastro-enterostomy with the Murphy button. A fourth case was reported by me in 1889—all having been highly successful. I am of opinion that the time is coming when many cases which are now being treated medically for gastric dilatation will be relieved with safety by the Loreta plan.

The matter of dealing with intraperitoneal rupture of the urinary bladder has been brought to notice as one of the valuable accessions to modern surgery. Geo. Heaton, of Birmingham, in the *Annals of Surgery*, June, 1896, has shown that of 31 cases in which the rent was sutured, 12 cases were successful, and 19 unsuccessful. He has collected 15 successful cases since 1896, the first being reported by MacCormac in the *Lancet*. The one condition upon which a fortunate outcome of these cases can be anticipated, is promptness in adopting operative methods. The method of repair is by the Lembert suture, as in dealing with the intestine, after which the abdominal cavity must be thoroughly sponged, packed, and drained.

The present status of ureteral surgery shows a remarkable growth of knowledge in this direction. In 1889, F. W. Kirkham, of London, successfully removed an ureteral calculus, and the operation was repeated by him in the following year. In 1889, Cabot, of Boston, reported a case of similar operation. Since that date various other authorities are on record as having succeeded in like manner. About the same time the matter of grafting the ureter to the intestine and bladder began to excite great interest. Wounds of the ureters were supposed to necessitate ablation of the corresponding kidney, and, so late as 1893, Kammerer, of New York, reported two instances of nephrectomy for wounds of the ureters during abdominal operations.

Uretero-intestinal anastomosis, objectionable for many reasons, but specifically because of the danger of direct infection of the kidney, soon gave way to end-to-end and lateral anastomosis. The observations of Van Hook, of Chicago, based upon experimental work, demonstrate the possibility of attaching the distal end of the divided ureter to the bladder wall, or of splicing the two ends of the tube, without interrupting its function. To Kelly, of Baltimore, is due the credit of having first successfully carried out Van Hook's plan, and of having established it as an accredited procedure.

In a notable article in the *Annals of Surgery*, January, 1897, J. W. Bovey, of Washington, D. C., refers to 12 cases of ureteral anastomosis, with 10 recoveries. In the two fatal cases the ureteral repair was shown by autopsy to have been perfect. His conclusions are so tersely and comprehensively stated, that I feel privileged to quote from them extensively.

"(1) Uretero-ureteral anastomosis is a perfectly feasible procedure. (2) Whenever possible, it is preferable to any other form of ureteral grafting, to nephrectomy, and to ligation of the ureter. (3) It should be done preferably by lateral implantation, or by end-to-end anastomosis. (4) Constrictions of the calibre do not usually follow. (5) Nephrectomy for transverse injuries of the ureter is an unjustifiable operation. (6) Simple ligation of the ureter to produce extinction of the function of the kidney is too uncertain to justify its practice. (7) Drainage is not necessary if the wound be perfectly closed and the tissues are throughout aseptic."

Surgical measures for relief of obstruction due to prostatic enlargement have excited intense interest within the past few years. While various procedures have not become established as to their relative value, sufficient light has been shed upon the subject to afford possible escape from the more complex conditions when routine palliative steps have proven inefficient. The choice of operation at the present time clearly lies between suprapubic prostatectomy and castration, after the plan described by J. W. White, of Philadelphia.

Professor Bellfield, of Chicago, in an article published in the *Edinburgh Medical Journal*, October, 1887, called attention to the desirability of the suprapubic route in approaching the prostate, and reported a successful case. Kummel, of Hamburg, in 1889, before the German Society of Surgeons, also advocated the measure as in every way superior to the perineal operation. About this time the suprapubic method was generally adopted in Europe and America, and was never called seriously in question until White, in 1893, in the *Annals of Surgery*, suggested castration as a means of relief for enlarged prostate.

Many operators have followed White's plan upon the assumption that it is attended with comparatively slight risk and with a reasonable degree of success. Cabot, of Boston, before the American Surgical Association, in 1896, joined issue with White. In an able paper, supported by a series of 90 cases gathered from various sources, he says that the operation is not devoid of risk, and as a matter of fact, is attended with even a higher mortality rate than is suprapubic prostatectomy. However, be this as it may, there can be do doubt that White is clearly justified in his claims: (1) That castration does produce a marked diminution in the size of the prostate. (2) That it will afford relief in as large a proportion of cases, with as small a risk, as any other operative method.

The *Boston Medical and Surgical Journal*, of March 25, 1897, contains an article by J. W. Elliott, which is an almost perfect reflex of our present knowledge regarding gall bladder surgery. He calls attention to the fact that there is no known medical solvent of gallstones, and adds much to previously accepted evidence showing that modern surgery offers a reasonable chance of cure. He maintains that in uncomplicated cases the gall-bladder, after evacuation, may be closed by immediate suturing, and that the common duct may be emptied of calculi and immediately sutured. He reports 5 consecutive cases of immediate suturing of the gall-bladder, 1 case of the hepatic duct, and 2 of the common duct, all of which were successful. The same author maintains that cholecystectomy is preferable to cholecystotomy where the organ has been the seat of destructive inflammation, as evidenced by thickened walls and extensive adhesions; or where recurrence of gall-stones has occurred. Cholecystectomy may be done as a secondary measure after cholecystotomy, for the purpose of closing a permanent biliary fistula.

The time seems to have arrived when the obstetrician must either equip himself surgically, or be prepared to avail himself of the services of a competent operator, as occasion requires. Ströbel has shown that craniotomy upon the living child must cease as soon as cesarian section is attended with an equally low mortality rate for mothers. He recalls the fact that at the hands of Leopold, Sänger, and Braun, the mortality of the cesarian section has fallen to 8, 4, and 3.22 per cent. respectively.

This chronicle of achievements and victories could be continued indefinitely, but I have already infringed upon your time and patience. I offer this incomplete and altogether unsatisfactory review of the original work done in the past ten years in verification of my opening statement—that progress, at least in one department of medical science, has taken another and very notable leap.

It is possible that some pessimist, under the guise of conservatism, will ask sententiously, has not the world been taught to think too much of surgeons, and have not surgeons come to think too much of surgery? Avowedly an optimist, and with a full measure of faith which has found expression in modest effort, I am disposed to treat this query in all seriousness. Doubtless there is justification for the prophecy that aggressiveness in the coming years will be controlled, and, along certain lines, restrained, by the positive knowledge which is begotten of experience and retrospect.

Rationalism, which long since supplanted empiricism, will in future advance upon a course that has been charted by the explorer. It is more than probable that certain limitations will be imposed upon the surgeon's jurisdiction by those who speak in tones of caution and command. The confession of Professor Senn, wherein he says: "I have made it a duty on my part to familiarize myself with the advances in the *technique* of aseptic surgery, and have the good fortune to perform all my operations in two of the best hospitals in Chicago, and in a fairly well equipped college clinic; and I am only too willing to confess that I never finish my day's work without seeing pus," is already a matter of public record. His warning in this connection is specially significant, and he emphasizes the recklessness of the remark so often made by men who ought to know better, "that the surgeon who understands his business, can make and treat wounds, which, if dressed properly, will heal without suppuration,'' closing with this comment: ''I but voice the sentiment of the vast majority of surgeons by making the statement that one of the limits of the art of surgery at the present time is the inadequacy of our available resources in furnishing wounds, even under the most favorable circumstances, absolute protection against infection.''

Others equally candid, and similarly clothed with authority, will insist that, in advance, a correct estimate must be placed upon the gravity of numerous humiliating sequels that mar the brilliancy of operative procedures. Less often, then, will there fall to the subject of an alleged successful operation a heritage of suffering and disappointment, an aftermath that has more than once left the stigma of reproach upon misguided, though honest, effort.

Finally, the consideration of a period so crowded with surgical activity has impressed me deeply, as it, doubtless, has impressed every right-minded operator, with the manifold exactions and responsibilities which perfected systems and high standards have imposed. Furthermore, there comes the suggestion of tireless effort, of eternal vigilance, and of constant heeding of the admonition so well expressed by Emerson: ''Explore, and explore, and explore. * * * Neither dogmatize yourself, nor accept another's dogmatism.''

DEPARTMENTS.

OBSTETRICS, GYNECOLOGY AND PEDIATRICS.

By WALLACE A. BRIGGS, M.D., Sacramento, Cal., and

WM. FITCH CHENEY, B.A., M.D., Adjunct to the Chair of Obstetrics and Diseases of Women and Children, Cooper Medical College, San Francisco, Cal.

The Treatment of Whooping Cough.—GROSS, in the *Jahrbuch f. Kinderheilkunde*, gives the results of his observations made in 874 cases; 832 were outdoor patients, the remainder were seen in private practice. The majority of the children were over two years of age. Fifty-two were nurslings. The internal medication comprised the use of bromide of potash, tincture of belladonna, codeine, quinine, antipyrine, phenacetine, antifebrine, and bromoform. Resina benjols was insufflated into the nose. In the majority of cases the treatment was begun when the paroxysmal stage was at its height. Bromide of potash and tincture of belladonna produced the least benefit. Quinine in some cases worked well; that is, when it could be given long enough and in sufficient doses. Small children refused to take it. It also acted unfavorably on the general well-being of the child, and the appetite under its use diminished. Of far greater value were antipyrine, antifebrine, and phenacetine, which were used in several hundred cases. The drugs were given in syrup, coffee, or water. Unfavorable results, such as depression and prostration, never followed the administration of any of the coal-tar preparations. Phenacetine seemed to possess the least value, but from 12 to 15 per cent. of the patients showed signal improvement under its use. Antipyrine gave the most favorable results.

Thirty-five per cent. of the cases showed marked improvement. None of the remedies mentioned above produced such an immediate change for the better, in convulsive attacks, as bromoform. Under its use vomiting and other complications were almost unknown. The convulsive character subsided almost entirely. The beneficial effects were noted in from 48 to 72 hours. The insufflation of resina benjols gave the best results of all the remedies used. In 60 per cent. there was a marked improvement in the severity of the paroxysms, with a shortening of the course. The beneficial effects were chiefly noted in diminishing the number of the paroxysms, the severity was controlled in a lesser degree. When no improvement follows four or five days' use of the insufflations, it is best to discontinue the treatment. The powder must be blown not only into the nose, but into the pharynx as well.—*Archives of Pediatrics*, May, 1897.

Pseudo-Meningitis after Influenza.—The case of a boy, aged 15, who had a severe attack of influenza in the spring of 1895, and again in November, is reported by TECCE (*Rif. Méd.*). The second attack set in suddenly while the boy was in the best of health. The temperature fell in two days, and the boy returned to work, but complained of headache. On the fourth day he fell down, lost consciousness, and was convulsed. The headache continued and was intense, and the patient became comatose. There was no delirium. Convulsions, with contraction of the upper limbs, rigidity of the lower limbs, and slight contraction of the neck muscles, were present. The pupils, urination, and defecation were normal throughout. The abdomen was depressed, and there was hyperesthesia of the vertebral column, and especially in the calves of the legs. The temperature was never very high. The pulse was rather slow. After about 20 days of treatment by cold to the head and calomel internally, the boy completely recovered, and has remained well since. There was no manifestation of tubercle in the child.—*Canadian Practitioner—Archives of Pediatrics*, May, 1897.

SURGERY AND ANATOMY.

By T. W. HUNTINGTON, B.A., M.D., Surgeon Southern Pacific Company's Hospital, Sacramento, Cal., and

GEO. B. SOMERS, M.D., San Francisco, Cal.

Chloroform.—The effect of chloroform upon the vital functions has been for many years a subject of hot dispute between various surgeons and pharmacologists. Some asserted that the danger of chloroform lay in failure of the respiration, others that the danger lay in cardiac failure. As with other discussions in medicine, the truth of the question as to whether chloroform causes death by respiratory failure or cardiac failure lies, as it were, half way between the two antagonistic forces; and, further than this, the somewhat startling statement may be made that it is not directly due, in the majority of cases, to either of these causes. On the contrary, the cause of death from chloroform is usually vaso-motor depression, whereby the arterioles allow the blood to pass freely into the great blood vessel areas, which are found in the capillaries and veins, and as a result the man is suddenly bled into his own vessels as effectually as into a bowl.—DR. H. A. HARE, in the *Therapeutic Gazette*, February 15, 1897.

Treatment of Varicose Veins.—DR. JOHN O'CONNOR advocates radical measures in severe cases. His procedure is as follows: The limb having been shaved and disinfected from Poupart's ligament to the ankle, a 2-inch incision is made over the saphenous opening, and the internal saphenous trunk is doubly ligated and divided. If no varicosity is present above the knee, the wound is closed, and dressed at once with iodoform gauze. If the femoral portion is affected, after ligation at the saphenous opening, the vein is dissected up, and its tributaries seized with pressure forceps and ligated. In nearly all cases if varices are present above the knee, there are also some below, consequently the incision is prolonged downwards directly over the vessel until the lowest limit of the disease is reached; the vein is then tied and divided below. In some cases an 18 or 20-inch incision is necessary.—*Medical Press and Circular*, February 10, 1897.

Tubercular Peritonitis.—In an article on this subject, HOLMES arrives at the following conclusions: (1) Tubercular peritonitis is a relatively common disease. (2) It is never a primary disease though it is usually impossible to find the initial focus. (3) Recovery follows laparotomy as a general rule, unless there is an initial focus remaining to keep up the disease. (4) This disease appears in three forms, the exudative form, the dry form, and the ulcerating form, and they are recognizable in the order named. (5) Macroscopical examination of the peritoneum is sufficient for a positive diagnosis. The demonstration of microscopical tubercles or the recognition of the bacilli are only confirmatory. (6) Puncture of the abdominal wall for diagnosis or for the removal of ascites and injection of air, fluid, or iodoform, is dangerous, and should not be practised. (7) Laparotomy, with iodoform gauze tamponade drainage, is the safest and most reliable treatment. (8) Laparotomy should be done as soon as there is a show of emaciation or when a relative diagnosis has been made. (9) A positive diagnosis can never be made before laparotomy.—*Annals of Gynecology and Pediatry*, October, 1896.

OPHTHALMOLOGY, OTOLOGY AND LARYNGOLOGY.

By WM. ELLERY BRIGGS, M.D., Sacramento, Cal.

Lesions of the Retinal Vessels, Retina, and Optic Nerves, Associated with Gout.—DR. CHAS. STEDMAN BULL discusses the leisons of the retinal vessels, retina, and optic nerve, resulting from the changes in the vessels by gout. A gouty retinitis or neuroretinitis exists which strongly resembles certain types of syphilitic retinitis, and which may or may not be accompanied by retinal hemorrhages and opacities of the vitreous. Gouty patients are prone to hemorrhages, venous obstruction, and irregularities of the circulation. Small linear and flame-shaped extravasations of blood may be scattered all over the fundus, appearing suddenly and involving one or both eyes. The changes to which the author calls especial attention are: (*a*) The changes in the walls of the blood-vessels of the retina, choroid, and optic nerve, including arteries, capillaries, and veins. (*b*) Retinitis of a peculiarly localized character, confined to the posterior zone of the fundus, with or without hemorrhages in the retina and vitreous, and characterized by a peculiar yellowish exudation, occurring in clearly defined patches. (*c*) Optic neuritis, generally with, but sometimes without, an accompanying retinitis. The author's conclusions are formulated as follows: (1) The changes in the fundus are always bilateral, though rarely symmetrical in the two eyes. The leisons may begin simultaneously in the two eyes, but this is by no means always the case. (2) The degenerative changes in the walls of the blood-vessels, both arteries and veins, are at first very minute, and are often overlooked. They must be carefully searched for, as they begin in the intima. (3) The general angiosclerosis, and the patchy exudation in the retina, cause marked impairment of central vision, but little impairment of peripheral vision, and the disease never ends in blindness. (4) The loss of central vision is always progressive up to a certain point. Improvement of the vision, after the retinal disease is established, cannot be expected, though in favorable cases the existing vision may be maintained. (5) Hemorrhages in the retina are rare, except in comparatively early stages of the disease. When the vessels lose their elasticity by reason of the increase in the thickness of their walls, due to the deposits, the vessels become stronger and more rigid, and hemorrhages are no longer to be feared. (6) The most marked feature in the ophthalmoscopic picture is the development of angiosclerosis in the vessels of the retina. This condition is confirmed by the microscope, and is seen to extend to the vessels of the optic nerve and choroid. (7) Another almost equally marked symptom is the peculiar yellowish granular exudation in the retina, located by the ophthalmoscope around the posterior pole of the eye, and generally leaving the macula intact until late in the course of the disease. This exudation is shown by the microscope to be mainly in the nerve-fiber layer, though found in all the layers except that of the rods and cones. (8) The changes in the optic nerve-fibers seem to be generally intraocular, but have been traced occasionally some distance back of the eye-ball.—*Medical News*, May 8, 1897.

DERMATOLOGY, SYPHILIS AND VENEREAL DISEASES.

Prophylaxis of Tumefaction of the Cervical Ganglia Consecutive to Eczema of the Scalp in Infants.—Antiseptic cleanliness and relief from the pruritus are the requisites, and STEINHARDT recommends for the purpose washing the child's hands and cleaning his nails every two hours, washing his playthings at the same time, and changing the clothes and bed linen frequently. The ezcematous regions should be moistened for an hour with fresh Goulard's lead water, and then the following salve applied: Salicylic acid 0.50 gm.; menthol 1 gm.; linseed oil and lime water āā 50 gms. externally.—*Semaine Méd* —*Journal American Medical Association*, May 8, 1897.

The Irrigation Treatment of Gonorrhea.—DR. FRED. C. VALENTINE says (*Clinical Recorder*): Recently published formularies seem to have created the impression that all gonorrheas are curable in eight days. As in all methods of treating any disease, circumstances, personal experience, the dexterity acquired by practice, influence the course of the ailment. Without reference to the mixed infections, consequently those in which the disease-provoker is the gonococcus alone, with irrigations without a catheter use mainly potassic permanganate. Goldberg shows that of acute gonorrheas, 60 per cent. are so cured within 10 days; 30 per cent. in 14 days; 5 per cent. required longer time because of violations of the injunctions regarding coitus and alcohol, and 5 per cent. were unexplained failures. American experience differs in that the failures are within 2 per cent. I have had 82 cases of genuine gonorrhea which recovered with three days, but all were patients who began irrigations when the meatus presented only a slight mucoid excess of moisture. On microscopic examination this was found to contain gonococci. The irrigator placed at nine feet from the floor gives adequate hydrostatic pressure to overcome the most resistent compressor. Lesser force, required for irrigating the anterior urethra, is easily obtained by the operator's thumb, with the present stop-cock. Perhaps less than 1 per cent. of patients complain of pain during irrigation. The exceedingly few who do, can be relieved by precursory injections of cocaine. The pain, however severe, usually ceases after the first irrigation. No drugs are used internally, except to meet other indications, as constipation, then giving cascara sagrada. There is no reason for patients to abstain from any kind of food during this disease. The habitual amounts of coffee, tea, or chocolate are permitted, but all carbonated drinks (vichy, seltzer, ginger-ale, root-beer, sarsaparilla, etc.), are strictly forbidden, as are all wines, liquors, beer, except to those who are accustomed to drink claret at meals. It is not evident that tobacco in any manner influences gonorrhea, therefore abstinence therefrom is not ordered. Refraining from moderate exercise cannot contribute to the patient's general health, therefore it should not be forbidden. The shower-bath is preferable while gonorrhea lasts. In the hands of those who irrigate carefully, no complications ever result. Catheter fever, a very frequent sequela to urethral instrumentation, is entirely avoided by irrigating the urethra or bladder without a catheter, according to the region invaded usually with potassic permanganate 1 to 6,000. It is difficult to imagine a chronic gonorrhea without changes in the tissues of the urethra. Should such a one present with too copious a discharge, or too sensitive a urethra for instrumental examination, the series of irrigations proposed by Janet would check it. Experience has shown that the urethroscope will always reveal at least one leison, without whose treatment the case will not yield to irrigations alone. The only proper method of protecting the garments is by dressing the glans with absorbent cotton soaked in mercuric bichloride, 1 to 6,000. If the prepuce is not large enough to hold the cotton, it may be made fast to the penis by a light bandage of gauze. The cotton and bandage worn should be burned after each irrigation and urination. The importance of deciding whether a gonorrhea is definitely cured, is beyond discussion. It will suffice here to cite the tests employed: (1) A week after every vestige of discharge has disappeared and the urine has remained perfectly clear, the patient is ordered, before retiring, to drink double the quantity of beer he used previous to his infection. If the next morning, or the following one, reveals no return of the discharge, the patient is dismissed for a week. (2) Then an irritant injection of silver-nitrate is made into the urethra. A discharge is produced thereby in from 4 to 16 hours. This discharge is examined for gonococci. If none be found, the discharge is allowed to recover without treatment. (3) Then the patient is instructed to use a condom in case he indulge in sexual intercourse, and to bring the condom for examination. If its contents are found to be free from gonococci; then (4) a urethroscopic examination is made. If this shows a perfectly healthy urethra, the patient certainly may be deemed cured. It often happens that the entire penis becomes exceedingly edematous after an irrigation. If the patient is not forewarned of this possibility, he may grow alarmed enough to desist from treatment, although the swelling is entirely painless and harmless. Indeed, in my opinion, it is this artificial edema which, though it be unperceived by the patient, renders the urethral mucosa an inferior culture-medium for gonococci; their nutriment thus being vitiated, or entirely destroyed, their proliferation is checked. The first irrigation is usually followed by an arrest of pain and a reduction of the heavy thick discharge to a slight, often colorless, excess of moisture. The patient must be warned that this trifling excess still can contain gonococci. Therefore, after touching the penis, the hands must be scrupulously washed, lest they carry infection to the patient's eyes, which may be rendered irretrievably blind in 48 hours, from the resultant gonorrheal ophthalmia.—*Medical and Surgical Reporter*, May 1, 1897.

MATERIA MEDICA AND THERAPEUTICS.

By WM. WATT KERR, M.A., M.B., C.M., Professor of Clinical Medicine, University of California, San Francisco.

Interstitial Injections of Methylene Blue in Epithelioma of the Face.—In the *Normandie Médicale* M. DUBARRY relates the following case: The patient, a woman fifty-seven years old, stated that eighteen years before she had noticed a pimple of about the size of a pin's head on the right side of the chin. For ten years it had remained in about the same condition. She had never been alarmed until one day when, on scratching the spot, which had itched for some time, she provoked a hemorrhage that lasted for four hours. She then consulted her physician. who advised a little operation, but the idea of an operation frightened her, and she allowed seven years to pass before again consulting a physician. During the last two years of this period the disease had greatly increased, and the pimple had attained the size of a fifty-centime piece; the itching also had become more frequent and persistent, and the spot often bled, especially when the patient removed the scabs which covered it. An operation was again advised, but as there was no suffering, the patient would not submit to surgical intervention. A year later the tumor had spread so rapidly that she decided to seek the advice of another physician, who, in view of the extent of the disease, strongly urged upon her the necessity of an operation, but she resolved not to have it done. Two years more passed, during which time the disease progressed so rapidly that she became alarmed, and again sought the advice of a fourth physician. At this time the scabby surface of the epithelioma occupied the entire right side of the chin from the hollow as far as the middle of the horizontal ramus of the maxilla, vertically it extended beyond the fold of the lower lip and beyond the body of the maxilla. The surrounding tissue was red, hard and edematous; and the lip was much thicker on the diseased side. The tumor was divided into four lobes, each one of which was covered with bloody scabs. When the author tried to separate them, a rather large quantity of bloody pus was discharged; he found also that the tumor was adherent to the periosteum of the maxilla. An operation was urgent, but, owing to the cowardice of the patient and the great loss of skin which would have resulted, and which would have been difficult to replace, the author resolved to try interstitial injections of methylene blue, although without any hope of success. M. Dubarry used a 10 per cent. solution in distilled water, and during the first applications he injected as much as 30 grains. Each time he saturated the entire diseased surface, pushing the needle as deep as possible into it, touched the periosteum, and throwing the liquid in all directions. This was repeated as often as it was necessary. The tumor and all the red tissue surrounding it became of a bluish color. Several times hemorrhages were provoked, but they were not alarming or of long duration, and a slight compression rapidly arrested them. After the first application the intense itching completely disappeared. During the first three months the author practised an injection regularly every two days, the average quantity of liquid injected being 15 grains. After a month of this treatment the tumor noticeably diminished in size. After the third month it had diminished one half; cicatrization occurred from the periphery toward the center. As recovery approached a more considerable resistance was felt; the tissues became more compact and denser. During the fourth and fifth months of the treatment only one or two injections a week were practised; the quantity of liquid used was also diminished in proportion as recovery advanced, and during this stage not more than two or three divisions of a Pravaz syringe were filled for the injection. In the beginning of this treatment, when the quantity of liquid injected was very large, the urine became of a decided blue color, which disappeared gradually, and it was sometimes 24 hours after the injection was practised before the urine regained its normal appearance. At no time did the patient feel pain in the kidneys or experience trouble on micturition. In June, 1896, the treatment was stopped, five months after the first injections. In February, 1897, there was a firm cicatrix on the spot where the epithelioma had been, which was crossed in all directions by fibrous bands which were very resistant to the finger and adhered to the maxilla. The cicatrix was scarcely larger than a two-franc piece. With regard to a complete recovery, the author states that, up to the present time, all the indications lead him to believe that recovery will be permanent. Even if a relapse should occur, the same treatment may be resorted to again. Methylene blue, says the author, seems to possess peculiar properties which are capable of making a profound change in the diseased cell or in the microbe which produces the affection. He is convinced that this mode of treatment will render great service, not only in superficial cancroids, but in the deep cancerous affections, notably of the face, provided, however, the diseased spot can be put in contact with the medicament. Each diseased element should be saturated with this therapeutic agent until the noxious principle is profoundly charged with it.—*New York Medical Journal, May 1, 1897.*

MEDICINE AND PATHOLOGY.

By ALBERT ABRAMS, M.D., Professor of Pathology, Cooper Medical College, San Francisco.

The Roentgen Rays in the Diagnosis of Pulmonary Consolidation.—BOUCHARD recently called the attention of the Academy of Sciences to the arrest of the Roentgen ray by pleural effusions. At the session held December 14, 1896, he reported that even slight condensation of the pulmonary tissue at the apex of the lung will cast a dark shadow. By examination of a number of tuberculous patients with the aid of the fluorescent screen, it was determined that the pulmonary lesions threw shadows which corresponded exactly with the area of consolidation as revealed by other means of physical examination. In two cases, light spots within the dark area revealed cavities, the existence of which was proved by auscultation. In other cases in which cavities were made out by auscultation, no light spots were seen in the shadow.—*Medical News,* January 30, 1897.

Tests for the Products of Albumin Digestion.—DR. D. D. STEWART suggests the following simple method for the products of the digestion of albumin acted upon by pepsin-hydrochloric acid in the removed stomach contents: The clear filtrate should be boiled to show the presence of undigested albumin. The resulting filtrate clouds in the cold on neutralization should syntonin be present. Albumin and syntonin should exist in mere traces at the height of digestion. To show the presence of proto- and hetero-albumose, the filtrate should then be treated with cold, saturated solution of sodium chloride and strong acetic acid. To remove deutero-albumose and traces of the other albumoses remaining, the neutralized filtrate is saturated while hot with ammonium sulphate. The resulting ammonium sulphate filtrate is examined for peptone by the biuret test, sufficient strong potassium hydrate solution being added to decompose all the ammonium sulphate present. A drop or two of a very weak cupric solution is then added.—*Philadelphia Polyclinic*, December 2, 1896.

MENTAL DISEASES AND MEDICAL JURISPRUDENCE.

By A. W. HOISHOLT, M.D., Assistant Physician, State Asylum for Insane, Stockton, Cal.

A New Definition of Idiopathic Diseases.— Dr. HELWEG, of Copenhagen, gives the following interesting definition of "idiopathic" in connection with mental diseases. "A disease is idiopathic when it is independent and governed by its own laws, whether it has taken its origin from some other disease or not. When an abdominal disease in a puerpera gives rise to an affection of the brain taking the form of amentia, it is said to be idiopathic, when it exists, not only while the fever lasts—increasing and diminishing with it—but takes an independent course beyond the duration of the fever. The pelvic disease may disappear after a few days, while the amentia originating from it may perhaps last a couple of years and pass through a series of phases terminating in recovery or becoming chronic. Is this amentia not idiopathic? Is it not an independent disease just as well as the heart disease, which, originating from an acute articular rheumatism, continues its existence long after the originating disease has disappeared? These diseases are deuteropathic in origin, symptomatic during their short incipiency; but during the rest of their long life they are idiopathic. A son does not lose his claim to be considered an independent being because he has been begotten by his parents; during his fetal life he is only symptomatic, but from the moment he is born he become idiopathic."—*Ugeskr. f. Lager—Hospitalstidende*, February 3, 1897.

Mental Stupor as a Pathological Entity.—DR. JAMES R. WHITWELL, of the West Riding Asylum, England, has made a clinical and pathological study of a series of cases of so-called mental stupor, and has brought forward considerable evidence in favor of the view, that these cases, or certain of them, may be due to a want of normal proportionate development in the circulatory and nervous systems; that, while the brain reaches the degree of development normal to the age, sex, and physique, the blood-vessels, and frequently also the heart, remain in their puerile condition, the aorta and other vessels throughout the body showing a condition of congenital narrowness, associated in some cases with a congenitally small heart. If the cardio-vascular condition be only temporary, and under proper stimuli continue to undergo its development, finally the normal vasculo-cerebral proportion is brought about, and the patient recovers from his mental stupor: If, however, a condition of permanent hypoplasia obtains, the case sinks into dementia, and is either carried off by some intercurrent acute disease, or, as is frequently the case, develops phthisis. His reasons are based on the *post-mortem* findings in certain cases of small heart and aorta; on the results ordinarily attributed to increased or diminished blood supply; and by the action of nitrites.—*Journal of Nervous and Mental Diseases.*

A Case of Sporadic Cretinism Treated with Thyroid Extract.—DR. L. HASKOREE reports a case of sporadic cretinism in which the patient, 38 years of age, was physically well built, while intellectually her abilities to accomplish anything were very discouraging. A goitre had gradually developed since her eighth year. Treatment with thyroid extract for six weeks showed an appreciable, and in fact, marked diminution in the size of the goitre. The disposition of the patient became one of restless activity, besides being also very irritable. Her powers of comprehension were clearer. As accompanying symptoms there was an increased rapidity of the pulse rate, slight rise of temperature, a copious perspiration, tremor of the upper extremities, and a diminution in the weight of the patient. In connection with the diagnosis of sporadic cretinism, the author expresses the opinion that cases should not be regarded as such, in which there is a congenital absence of the thyroid gland. *Wiener med. Wochenschr.—Journal Nervous and Mental Disease.*

Juvenile Paresis.—DR. ALZHEIMER reports, in detail, 3 cases of early paretic dementia, or developmental general paralysis of Clouston and others, which have come under his personal observation. Besides these he analyzes 38 cases found in the literature. He finds that syphilis plays a predominant, if not, indeed, an exclusive role in the etiology of the disease; that hereditary predisposition seems to favor it, and traumatism may be the immediate cause of the outbreak of the symptoms. Clinically, the tendency is towards a form of chronic dementia without delusions, though other psychic symptoms may occur, but, generally, as transient phenomena. The course and prodromal stage are rather longer than in the adult type. The paralytic symptoms generally appear early, and are prominent throughout. Hemiplegias, paralytic strokes, tabetic symptoms, optic atrophy, etc., are relatively rather frequent. The microscopic *post-mortem* findings are similar to those in other forms, and the same is true of the finer lesions. In 2 of his own cases he found pronounced degeneration changes in the basal ganglia. It is the writer's opinion that one would not be justified in making these cases a special clinical type, as the youth of the subjects is the only feature not met with in different cases of paresis of older persons. This early form of dementia paralytica, he thinks, however, throws a certain important light on the pathology. The usual causes other than syphilis, to which paresis is attributed, such as grief, care, overstrain, the struggle for existence, fail here. The paresis, occurring at a time of life when physical and mental strain are at a minimum, makes the influence of syphilis all the more marked. The developmental changes, to which the paresis has been ascribed by Clouston and others, can hardly play a part as early as the ninth year (at which age some of the cases began), and, therefore, Alzheimer rejects these as an especially important factor.—*Allg. Zeitschr. f. Psych.—Dublin Journal of Medical Science.*

Consanguineous Marriages.—DR. S. SALOMONE-MARINO read a paper on this subject at the last Congress on Internal Medicine in Rome. The author collected all available information upon consanguineous marriages and their results, occurring in the township of Borgetto, during a period of 90 years: noting the offspring, its death-rate, and its state of health, and comparing these with similar statistics for other marriages. His conclusions are that, except in cases in which a hereditary disease exists in the family, no injurious consequences are to be apprehended; and that such marriages do not differ from others in fecundity, or in the health or mortality of the issue.—*Il Policlinico—Dublin Journal of Medical Science*, December, 1896.

FORMULÆ.

By H. E. SANDERSON, Ph.B., M.D., Assistant Physician State Asylum for Insane, Stockton, Cal.

Drink in Bright's Disease.—SEMMOLA recommends the following:

Sodii iodidi	gr. xv
Sodii phosphat.	gr. xxx
Sodii chlorid.	gr. xc
Aquæ	℥ xxxvi

S.—To be drunk daily, alone, or with milk.—*Practitioner.*

Gastric Hyperacidity:

Methyl blue, in 1 to 3 grain doses, daily, for several days. Then discontinue for three or four days, and then again treat as before. Said to relieve pain, improve digestion, and suppress tendency to vomiting.—*Medical Progress.*

PUBLIC HEALTH.

By W. R. CLUNESS, M.A., M.D., San Francisco, Cal.

Mortality for March, 1897.—The deaths registered in 48 town districts of the State, during the month of March, in a population of 735,055, correspond to an annual rate of 17.52 a thousand, the total mortality having been 1077. 62 deaths were due to zymotic diseases, giving an annual rate of 1.008 a thousand. Of these, 17 were due to diphtheria, 0 to cholera infantum, 12 to typhoid fever, 3 to diarrhea and dysentery, 5 to cerebro-spinal fever, 9 to whooping cough, 8 to croup, 1 to intermittent and remittent fevers, 2 to scarlet fever, 5 to measles, and 0 to small-pox. 328 deaths resulted from diseases of the respiratory organs, giving an annual rate of 5.28 a thousand. Of these, 194 were due to consumption, 99 to pneumonia, 22 to bronchitis, and 13 to pulmonary congestion; the rate being, for consumption and pneumonia, 3.12 and 1.60, respectively. 46 deaths resulted from diseases of the heart, 6 from alcoholism, 1 from erysipelas, 43 from cancer, and 538 from causes not given. The average annual death rate from all causes occurring in the ten largest cities and towns in the State, and representing a population of 607,000, was 17.76. The highest rate for the month occurring in cities having a population of 10,000 or more inhabitants, was reported from San Francisco, the lowest from San Diego.

METEOROLOGY.

Summary for March, 1897.—*Temperature.*—The normal temperature of the State for March is 53.0°; the average for March, 1897, was 48.0°, a departure from the normal of −5°. The highest monthly mean was 66.0°, at Volcano Springs; the lowest, 17.0°, at Bodie. The maximum temperature was 98°, at Volcano Springs, on the 25th. The minimum, 14° below zero, at Bodie, on the 8th. The absolute range for the State was 112°. The greatest monthly range was 62°, at Truckee; the least, 15°, at Port Los Angeles.

Rainfall.—The normal precipitation for the State for March is 3.43 inches; the average for March, 1897, was 3.98 inches, a departure from the normal of +0.55 inches. The greatest monthly precipitation was 20.43 inches, at Sissons; the least, nothing, at Palm Springs, Needles, Ogilby, Salton and Volcano Springs.

Wind.—The prevailing direction of wind ranged from S.W. to N.W.

Unusual Features.—The unusual meteorological phenomena were the low mean temperatures for the month with severe frosts.—JAMES A. BARWICK, Director California Weather Service.

California—Tabular Statement for March, 1897.

STATIONS.		TEMPERATURE.					RAINFALL.		Mean Relative Humidity	WEATHER.			WIND.	FURNISHED BY—
	Mean	Highest	Lowest	Mean Highest	Mean Lowest	Mean Daily Range	No of Days Rain Fell	Total Rainfall		No. of Clear Days	No. of Fair Days	No. of Cloudy Days	Prevailing Direction.	
Auburn	Cal.	45.0	73	30	------	------	------	----	8.05	------	----	----	----	N.W.	Southern Pacific Company.
Berkeley	"	48.0	62	35	54	42	12.0	14	5.97	85	14	7	10	S.	Obs. University of California.
Colfax	"	43.0	68	31	------	------	------	----	8.65	------	----	----	----	S.E.	Southern Pacific Company.
Coronado	"	56.0	70	40	62	48	14.0	8	1.53	71	18	4	9	N.W.	Hotel del Coronado.
Eureka	"	45.0	62	29	52	38	14.0	26	9.85	81	5	16	10	N.W.	Weather Bureau Dept. Agr.
Fresno	"	49.0	71	31	58	39	19.0	10	1.64	68	19	6	6	N.W.	" " "
Keeler	"	50.0	77	36	------	------	------	----	0.13	------	----	----	----	N.	" " "
Los Angeles	"	53.0	78	38	62	44	18.0	7	2.31	74	11	13	7	S.W.	" " "
Monterey	"	52.0	68	36	------	------	------	----	3.70	------	----	----	----	S.W.	Southern Pacific Company.
Napa	"	47.0	69	28	58	36	22.0	12	5.88	84	15	10	6	S.W.	Station Napa Insane Asylum.
Oakland	"	48.0	64	36	56	41	15.0	18	6.00	81	7	13	11	N.W.	Chabot Observatory.
Paso Robles	"	48.0	69	33	56	39	17.0	6	2.88	------	18	6	7	N.W.	Agr. Experimental Station.
Red Bluff	"	48.0	67	32	56	40	16.0	13	1.99	61	10	19	12	S.E.	Weather Bureau Dept. Agr.
Sacramento	"	49.0	70	36	57	42	15.0	13	2.54	56	10	13	8	S.W.	" " "
San Bernardino	"	51.0	79	30	62	40	22.0	12	3.41	------	12	13	6	S.W.	A. K. Johnson, M. D
San Diego	"	54.0	70	40	60	48	12.0	8	1.53	71	18	4	9	N.W.	Weather Bureau Dept. Agr.
San Francisco	"	49.0	63	39	54	44	10.0	15	4.56	74	13	8	10	W.	" " "
Santa Barbara	"	53.0	76	34	62	44	18.0	5	2.73	65	22	5	4	W.	Hugh D. Vail.
Santa Cruz	"	49.0	70	30	61	37	24.0	13	4.86	------	16	7	8	W.SW.	W. R. Springer.
Yuma	A. T.	58.0	86	39	------	------	------	3	0.48	------	24	7	0	---- ----	Weather Bureau Dept. Agr.

Blank (----) indicates data missing.

Occidental Medical Times.

COMMUNICATIONS are invited from all parts of the world. When necessary to elucidate the text, illustrations will be furnished without cost to the author.

SACRAMENTO: MAY, 1897

THE MEETING OF THE STATE SOCIETY.

The twenty-seventh annual meeting of the State Society promised to be one of the most successful in its history. San Francisco was the place of meeting, and here certainly the largest possible attendance might be expected. The programme contained a wealth of material, some of which was decidedly good; over 40 papers were listed, a greater number than ever before. Special features of a novel kind, illustrative of the very latest progress in various branches of medicine, were provided for two of the evening sessions, and yet, in spite of all this, the meeting was not as successful as might have been anticipated. It is not always easy to fix the responsibility for failure, nor can it be said that individual responsibility may be asserted. It is, however, plain that a programme of the magnitude of that presented at the last session could only be carried out by using every available minute of three daily sessions.

We have always held that by far the most valuable feature of the State Society meetings are the discussions, and, apart from papers on abstract subjects and of high scientific merit, the average writer does best who elicits a full and intelligent discussion of the theme he has presented. Discussions consume time. Their conduct demands, on the part of the presiding officer, promptness and firmness of decision. No speaker should be allowed to trespass on the rights of others, and, if the paper exceeds the constitutional limit, its reading should promptly be terminated. Conceding that these factors are essential to the conduct of a successful meeting, we hasten to say that their achievement and combination will never be reached in the State organization of California, nor in any other of which we have any knowledge. It therefore remains to provide for that which is possible. A somewhat extended experience has convinced us that four papers, with a fair amount of discussion upon such, are amply sufficient for a single session. Six papers can be included, by good management and firmness on the part of the chair, but papers in excess of that number can only be read at the expense of the discussions thereon. The Executive Committee has recognized the difficulty at the present session, and has recommended that, in future, fewer papers be placed upon the programme. In this recommendation we heartily concur, and it will be well for future Committees of Arrangements to bear it in mind. It is discussion which mainly enlivens the sessions and gives them color, because into these discussions is projected the individuality of the speakers, and often their personal experience and predelictions in a manner that is rarely seen in a set paper. It is the absence of discussion that gives rise to the complaint of dulness and of monotony in our meetings, and certainly every effort should be made to stimulate this most valuable coloring.

San Francisco, containing as it does a large proportion of the membership, would be expected to furnish a good attendance at the sessions, yet it is never certain, in advance, that a meeting in the metropolis will be a success, and it is notorious that many of them have been flat failures. The remedy for this is obviously rotation in the places of meeting. Experience has shown that some of the best meetings in the history of the Society have been held outside of San Francisco. The Society has, therefore, done well in selecting Fresno for the session of 1898. As this will be the first meeting that has been held in the center of the State, a profound responsibility rests upon the Fresno County Society, and upon the San Joaquin Valley District, in general.

NOTES.

Board of Examiners.

The new Board of Examiners of the Medical Society of the State of California, consisting of the following gentlemen, J. Henry Barbat, Elmer E. Kelly, W. E. Hopkins, Washington Dodge, F. B. Carpenter, A. P. Woodward, Chas. C. Wadsworth, have organized by electing J. H. Barbat, President, and C. C. Wadsworth, Secretary. The office of the Board is 1104 Van Ness avenue.

Water Fronts.

In the *Bulletin*, published by the State Board of Health of Tennessee, there is a suggestive article on the value of water fronts for park purposes. The writer says: In the Eastern cities much regret is now expressed at the lamentable want of foresight manifested a generation or two ago by the rulers of the same. These great cities have, as a rule, splendid water fronts, a fair proportion of which should have been reserved for public parks. Not looking ahead, these magnificent beauty spots have been, step by step, appropriated for wharves and piers. Whenever a city or town has a water front, whether on river, ocean, or lake, a liberal portion should be reserved for public use. This is a sanitary and esthetic axiom, which should be repeated and urged until the people insist on its observance. It is to be noted that San Francisco has a very extensive water front, which is being improved with a long sea-wall, but no provision has been made for pleasure piers. In spite of the numerous public parks in the city, one may see on Sundays and holidays many hundreds of people wandering along the wharves, attracted by the shipping and marine view. Undoubtedly a pleasure wharf or two, at favorable locations on the city front, would be popular and sanitary.

SOCIETY PROCEEDINGS.

SACRAMENTO SOCIETY FOR MEDICAL IMPROVEMENT.

Regular Meeting, March 17, 1897.

The President, F. B. Sutliff, M.D., in the Chair.

Election of Officers.—The following were elected officers for the ensuing year:

President	W. J. Hanna
Secretary and Treasurer	F. M. Reith
Directors	G. L. Simmons, H. L. Nichols, W. A. Briggs, W. J. Hanna, and F. M. Reith.

Shock.—Dr. G. A. White read a paper upon this subject. He said: Shock, like inflammation, is a complex state that cannot be dismissed with a definition. The two principal theories as to the nature of shock are based on certain functional disturbances of the vascular and nervous systems respectively. Many surgeons regard shock as a sort of heart failure—a temporary paresis of the muscles of the heart. It may be of any degree of severity, from the mild, scarcely appreciable disturbance of the mental faculties to the most profound depression and death. Regarding the treatment of shock, as in many other things, an ounce of prevention is worth a pound of cure. To apply heat, to check hemorrhage, to stimulate the heart, and to promote nervous and mental rest, are plainly indicated. The reliable therapeutic agents in heart failure are: Morphine, atropine, digitalis, strychnine, and whisky.

Dr. H. D'Arcy Power: As far as I am able to see, in the question of etiology of shock and its pathology, there appears to be comparatively little doubt as to what the primary condition is. We must remember that the chief and only agent which will keep the heart going is the presence of intracardial pressure, and that if you remove that pressure the heart ceases to act. The primary cause of failure of the heart is the absence of fluid in its cavities. There is only one explanation to it, namely, that the irritation of the surface by whatever cause, gives rise to nervous irritation, which, by reflex action through the sympathetic system, produces temporary paralysis of the muscular coats of the arteries. In consequence of this you have bleeding of a man into his own vessels. The sluice gates which regulate the arterial system are raised and the blood passes into the venous system with loss of that blood pressure which is the stimulus of the heart, and without which the heart fails to respond. That is the essential pathology. There can be but one rational treatment of shock, and that is to raise the blood pressure and bring back the circulation; the means to which have been ably pointed out by the author. The use of injections into the rectum, of extravenous injection, or injection into the cellular tissue of normal saline solution, are useful and rapid methods of raising blood pressure. Atropine is a more valuable remedy in shock than digitalis, and should be combined with strychnine. Digitalis is useful afterwards. The use of $\frac{1}{50}$ to $\frac{1}{25}$ of a grain of atropine along with $\frac{1}{20}$ of a grain of strychnine is justifiable. That, with the saline solution, seems to be the most rational treatment.

Dr. T. W. Huntington: I was impressed by that feature of the paper, having reference to the length of time occupied in an operation, as one of the special causes that enhance surgical shock. I wish, however, to say that I have come to think that there are as great or greater dangers along the pathway of hasty operations as from the afore-named cause. In other words, many lapses and accidents are the consequence of undue haste, and I am disposed to say that the greatest good to the greatest number is obtained in the occupation of all the time that is dictated by the good judgment of the average surgeon. As an illustration of this point, perfect control of hemorrhage is not in any sense compatible with rapid closure of the wound, and to this end, I am in the habit of continuing etherization, and extending the time of operation until I am satisfied that the patient's interest in this respect has been perfectly conserved. I wish to offer a suggestion, as regards the using of atropine, which was advocated by the opening speaker. For some time I was in the habit of resorting to this drug, but in the main failed to observe the marked effect which others have ascribed to it; hence I have now practically abandoned its use. In place of it I have endeavored to fortify the heart with strychnine and by increasing the amount of fluid in the circulatory apparatus, by transfusion of normal salt solutions subcutaneously. In a recent case of double amputation of the legs, in which the patient was perfectly exsanguinated, I injected $3\frac{1}{2}$ quarts of salt solution during the operation. The patient withstood the shock and is now well. The one great precaution in adopting this step is to see that the salt solution is perfectly sterile, otherwise ugly localized septic conditions will almost certainly complicate the future history of the patient. I wish to allude to another matter, the avoidance of shock by controlling hemorrhage. This step is too often overlooked and inefficiently attended to by those who have first charge of the patient. The rule should be to apply a firm constricting band above the point of injury, and a second or a third when necessary to staunch the blood flow completely. I believe if this were impressed more fully upon the minds of those liable to have charge of the injured, temporarily, we should have fewer deaths from shock.

Dr. J. H. Parkinson: I think many operators spend more time that is consistent with good judgment. We all know that in preanesthetic days rapidity in operation was everything, and that the man who was the most rapid operator was the best. The age of anesthesia and of antiseptics came and there was a still further prolongation in the time of operation owing to the methods and preparations. There is no question in my mind that there is a double shock—first the shock of the anesthetic and next the shock from prolonged exposure. I have now had considerable experience in administering ether and I know that it is often given so as to produce prolonged anesthesia, and of a profound degree when this is unnecessary. I agree with Dr. Huntington that there should be no undue haste, but the tendency has been to work too leisurely.

Dr. G. L. Simmons: It has puzzled me sometimes to explain how compartively minor blows will kill and why they are fatal in one case and not in another. In this connection I remember the case of a strong and well nourished boy of 15, who was playing at base-ball and received the ball of the pitcher directly under the ensiform cartilage He died before I could reach him, and a *post-mortem* examination revealed no evidences of disease or rupture or malformation. A well known surgeon of San Francisco once asked me: "How do you describe shock?" The answer that it is a severe reflex disturbance or paralysis of nerve centers hardly satisfied the inquirer, but a better definition is difficult to give. I am gratified that the author has introduced this subject, and I am sure we will all profit by his observations.

DR. THOS. ROSS: It is claimed that, as far as the prognosis in regard to shock is concerned, a majority of persons recover who have a fall of less than one degree in temperature, and that a majority of cases die if they have a fall of three degrees in temperature. The most severe case of shock that I have ever seen recover was one from a small penetrating wound of the abdomen, made with a penknife, in which there was no injury to the intestine.

DR. W. A. BRIGGS: The condition seems to me to be a reflex vaso-motor paralysis, and, if we consider it so, it seems to me that the first thing to do is to diminish the conductivity of the nerves; in other words, to diminish the centripetal impulse of the nervous system. This, it seems, is best done by using morphine before the operation and during the existence of shock. I believe morphine is valuable in this way, and in stimulating the heart. The second indication it seems to me would be to contract the arterioles, and this can be done, as Dr. Power has said, by atropine, which I believe is a useful remedy; I have seen good effects from it a number of times. Digitalis acts slowly, and I have never seen any beneficial influence from it except in prolonged shock. In the third place, fill the blood vessels. This can be done by injections, as has been already stated—intestinal, venous, or arterial. In the fourth place, maintain the cerebral circulation, which can be done by position. In the fifth place, maintain the body heat. Refrigeration is one of the dangers of shock, and should be carefully guarded against. As regards fear of air getting into the blood vessel, as has been expressed by one member of this Society; very recent experiments have shown that, as far as the lower animals are concerned, this is certainly groundless. Repeated experiments have been performed by German and, I think, Russian physicians in this direction, showing that a considerable amount of air can be injected into the veins without any serious result. I think it requires more than 600 grammes of air injected into the jugular vein of a horse to produce death.

DR. H. L. NICHOLS: The doctor particularly refers to the shock following surgical operations, but we have, of course, shock from accidents, hemorrhages, etc., which are as difficult to treat as if from surgical operations; gunshot wounds, electric shocks, and from uterine hemorrhage especially. I have found, in addition to some of the remedies given by the doctor, that injections, under the skin, of sulphuric ether have a wonderful effect, especially in the shock from uterine hemorrhage. During the discussion a case came to my mind that occurred some years ago. I was called one morning to attend a boy, 8 or 10 years old, who was accidentally shot on his way to school. The pistol was loaded with small bird shot, and he received the charge in the chest. Several physicians were sent for—Dr. Simmons being preferred—but he was out of town at the time. I examined him closely, and to my mind the boy was apparently dead, and I believed the heart had been punctured, there being neither pulsation nor respiration. The doctors who had arrived before me had applied remedies, but unsuccessfully. We stayed quite a while and I gave my opinion that he was dead, and that there was no use attempting anything further. I think Dr. Haswell agreed to remain awhile. The lady at whose house he was did not seem to have much faith in our opinions, but she had unlimited confidence in Dr. Simmons, who, if there, she was confident could bring the child to life. Finally, the doctor returned, and, finding the order on the slate, drove to the house, and I learned afterwards that about the time he entered the house and looked at the patient he discovered a slight effort at respiration, and of course did everything that could be done to resuscitate the patient. His efforts were successful, and the boy lived, and the lady's faith in the doctor's power to raise the dead was fully confirmed. Since then I have been more careful about giving positive opinions of death after shock.

DR. L. A. HARCOURT: The paper just read was confined exclusively to surgical shock. But very little has been said about cases of shock from accidents, which it seems to me are more numerous. In those nothing can be done to prevent shock, as it has occurred before the surgeon is called in, and what shall we do then? The remedy, if there is one, has not been touched upon at all. A great deal has been said about blood pressure. There is a certain amount of blood in the system, and if there is no lesion of a vessel or of the circulatory apparatus, I would like to know where the blood goes? If the system is not depleted by hemorrhage, then there is as much blood as there was before. No explanation has been given of that. The use of saline injections are most useful where hemorrhage has occurred If the blood still remains in the vessels, and there has been no lesion by which it escapes from them, it must be evident that there is as much in the system as before, and not so much necessity for the saline solution. I would like an explanation on that point. Nitro glycerine and brandy, hypodermically, would have some effect in sustaining the heart's action. Another feature that has been overlooked is, that the effect produced depends somewhat upon the condition of the person at the time of the accident. A very slight injury, under certain circumstances, will produce shock. A very grave one might produce none. It is an absolute fact that men will sometimes lose an arm and remain unconscious of the fact until the blood begins to drip; so that the condition of a person previous to or at the time of the accident has considerable to do with determining the extent of the shock.

DR. POWER: Dr. Harcourt has evidently overlooked the physiology and anatomy of the circulatory system. The fact is that the capacity of the veins and arteries are much greater than what is necessary to hold the blood at any given time. The size and calibre of the whole apparatus of circulation is quite considerable, and only a certain portion is used owing to the contraction of the muscular coats of the vessels. If these are paralyzed, the capacity of the circulatory system is enormously increased. The blood flowing into the relaxed arterioles and capillaries depletes the large arteries. There is always an amount of pressure in the arteries. Unless you have a certain amount of pressure the heart stops.

SAN FRANCISCO COUNTY MEDICAL SOCIETY.

Regular Meeting, February 9, 1897.

The President, F. B. CARPENTER, M.D., in the Chair.

New Members.—ERNEST BARRY, M.D., and W. M. S. BEEDE, M.D., were duly elected members of the Society.

Obstetrical Operations.—DR. H. KREUTZMANN read a paper upon this subject.

DR. J. H. BARBAT: Recently, Dr. Carpenter and myself had a case of placenta previa, in which the hemorrhage was severe. The vagina was packed with four yards of gauze. This was removed after 30 hours and the case terminated very favorably. The os was dilated forcibly. Albuminuria in pregnant women requires careful watching, and I favor the rapid emptying of the uterus after the first convulsion. It is best to be on the safe side, both for the mother and the child.

DR. W. F. CHENEY: These are extremely interesting cases. I cannot agree with Dr. Barbat that, in every case, after the first convulsion, we should empty the uterus. Should a convulsion occur at the sixth or seventh month, the question arises whether we should not wait till the child has become viable, if there be no recurrence of the spasm. Dr. Gibbons and myself recently had a very interesting case of prolapsed cord which turned out successful, and in five cases of placenta previa, in consultation and in private practice, I was fortunate enough to have good results.

DR. E. G. FRISBIE: During the past year I have had three cases of albuminuria in pregnancy. I am inclined to agree with Dr. Barbat that, as soon as convulsions appear, we should empty the uterus, as the safety of the mother takes precedence over that of the child.

DR. C. G. KENYON: In cases of placenta previa it is always good practice to attempt podalic version. The pressure of the child's body will then practically control the hemorrhage. Morphine should be given in sufficient quantities to control the convulsions. It has lately been specially recommended for this purpose by a physician of Rochester, N. Y.

DR. C. G. KUHLMAN: In connection with the case of adherent placenta, I may mention that I once had, in Portland, Oregon, a case, in which I inserted the hand into the uterus and I very soon had eversion—the result, no doubt, of uterine inertia. Since then I have refrained from traction in cases of suspected adherent placenta.

DR. G. W. DAVIS: I should not accept an obstetrical case unless it had been under my care for two months previous to the date of confinement. *Post-partum* convulsions are the only kind I have witnessed. Of course, where albuminuria is present treatment should be adopted to prevent the convulsions.

DR. KREUTZMANN, in replying, said: One-third of a grain of morphine was given in the case mentioned. The urine of the child having convulsions was not examined chemically. He had written this paper with the object of eliciting a discussion, as he felt that the subject of obstetrics was a very important one and that, even in the schools, proper instruction therein is lacking.

The Use, Abuse, and Sterilization of Catheters and Bougies.—DR. M. KROTOSZYNER read a paper upon this subject. He said: It is impossible to say how long catheters have been in use. In ancient times they were used about as now, so far as metal instruments are concerned. But the shapes then were not adapted to the urethra, and only skilled manipulators used them. Flexible catheters were first described about two centuries ago. Gueterbock first gave us exact knowledge of them about the end of the seventh century. But they were made of wax and other perishable material, and very unsatisfactory. About 100 years ago, a Russian surgeon, Theden, made catheters of rubber. They consisted of silk-woven wire net, shellaced with rubber varnish. This was much improved by Mr. Bernard, of Paris, a goldsmith, who used a silk texture covered with rubber varnish. This is the nucleus of all improvements made in the past century in flexible catheters and bougies. Lately American invention has rivaled both the French and English manufacturers. The old French scale increased $\frac{1}{3}$ m.m.—a new one adopted by Guyon increases only by $\frac{1}{6}$ m.m. His new shape seems an improvement. The instruction generally given to depress the upper end of the sound when the point reaches the membranous urethra is not always good, because you cannot always know exactly when the membranous portion is reached. Delafosse and Posner seem to have the best idea: Take the penis in the left hand, pull it and introduce from the side of the patient with the point downward till you reach a resistance, which is generally well marked, at the fossa bulbi. As soon as you feel this resistance, raise the sound to the median line, the point being pressed forward. To relieve the pressure of the point against the suspensory ligament, it is well to press gently on the pubes with the left hand. Then the point, by law of gravity, should pass easily into the bladder. Retention with prostatic hypertrophy, physicians generally treat with the flexible catheter; and at Bergmann's clinic metal instruments are considered dangerous. Sir Henry Thompson and Keyser's work on genito-urinary diseases, advise the use of silver catheters in such cases, and my experience bears out their assertions. It may be well to try a flexible catheter first, but one can do at least as much harm with the flexible as with the metal instrument. The imperfect surfaces, so common on account of cracks, etc., in soft instruments, make them objectionable. We even hear of accidents in which a piece of a flexible catheter has been broken off and left in the bladder. Our present methods of sterilization are very imperfect. The so-called button-sound is a most important instrument in urethral troubles. With it you readily detect sensitive areas, site of strictures, lengths of various portions of urethra, etc. I generally use No. 18–20, by Charrière. Though we have, as yet, no perfect method of sterilization, I think that of Dr. Frank, of Berlin, is the best, up to date. He uses formaline vapor—a 40 per cent. watery solution of formaldehyde. As stated, many thorough tests made by German experimenters show that all makes of flexible instruments are of uncertain quality, so far as they have power to resist the corroding effects of our present methods of sterilizing—the English makes seem the best. It seems that catheters and bougies ought to be sold as thermometers are, with a certificate of perfection. With some radical laws, or some established precedent, we should soon see all instruments of uncertain quality disappear from our markets.

DR. L. BAZET: Cleanliness is almost the most important requisite in our work. We should be clean in the field of operation and clean in our persons. When using the sound—and I always use a rubber one first—I disinfect the meatus with a bichloride solution of 1:25000. If necessary to reach the prostate I use a conductor, and if need be, larger sounds in increasing sizes. Boric acid solution in the bladder is very useful. Two years ago I brought 15 or 20 dozen rubber instruments from Europe, and they are still good. They have been used often, and have passed through a sterilizing process many times. They are of French make, and I think fully as good as Dr. Krotoszyner's German metal sounds. It is absurd to think that one country can make much better instruments than another, even than in America. The curve of Benicke is based on anatomical measurements and cannot be altered.

DR. C. G. KENYON said he used the short curve because he had become wedded to it through years of satisfactory use. He prefers metallic catheters to the soft instruments.

DR. E. G. MCCONNELL: We can welcome both French and German instruments, as no one kind answers for all purposes. Tiemann's soft rubber catheters boiled in a boracic acid solution are the best that we can do at present. As Dr. Bazet says, we must follow the anatomy of the parts, and also allow the sound to enter by its own weight.

DR. C. G. KUHLMAN: I do not know how often Dr. Krotoszyner has infected the bladder with sounds, bougies, etc. My own experience is devoid of such results. Thompson, Lisfranc, Bigelow, and Otis in passing sounds, catheters, etc., hundreds of thousands of times, fail to report a single case of infection. In view of this, is a physician unscientific in using instruments (as heretofore) unless possessed of the formidable array of instruments presented here to-night?

DR. KROTOSZYNER said he was glad Dr. Kenyon had called attention to the fact that it is easier to pass a large catheter in retention of urine. He was sorry that any remarks of his own on German manufacturers should have aroused Dr. Bazet's feelings. Personally, he also used French instruments.

THE MEDICAL SOCIETY OF THE STATE OF CALIFORNIA.

Twenty-seventh Annual Meeting, held in San Francisco, April 20, 21, and 22, 1897.

FIRST DAY—TUESDAY, APRIL 20—MORNING SESSION.

The twenty-seventh annual meeting of the Medical Society of the State of California was held in Golden Gate Hall, San Francisco, the Society being called to order by the President, HENRY GIBBONS, JR.

Address of Welcome.—WM. FITCH CHENEY, of San Francisco, Chairman of the Committee of Arrangements, said: *Members of the Medical Society of the State of California, Ladies and Gentlemen:* The time has arrived for the opening of another of the annual meetings of this Society. It has been my pleasure to aid in the arrangements for this meeting, and it is my pleasure now to welcome you to this place. This ought to be a memorable meeting, for the programme includes much that is of vital importance to all of us in the daily work we have to do. Your committee that has had charge of arranging the different features of the programme, which committee I have the honor to represent, believes that good value is offered you for your time spent this year. In the way of attraction we offer you the most attractive thing in the city—I refer to the magnet of Dr. Barkan. We offer you also in this programme the best that culture can produce, at least the best it can produce in test tubes and bottles. Finally, we offer you the substantial truths most acceptable to scientific investigators, as the Palace Hotel supplies them. With these and other features before us we feel justified in predicting your approval of this meeting, and can heartily congratulate you that you are here.

Annual Address.—The annual address [published at p. 225], was then read by the President, HENRY GIBBONS, JR., of San Francisco.

Committee on Medical Topography, Meteorology, Endemics and Epidemics. The report of this committee was read by the Chairman, MAX RICHTER, of

San Francisco, his subject being "The Formation in California of Colonies for Consumptives." He said: Protection from consumption has become a war-cry of the health boards. Since Koch's discovery of the bacillus of tuberculosis an earnest and pressing means of combating this dread disease has become an increasing problem in all enlightened countries. Much work has been done by the health authorities in many places looking to the elimination of the disease. Stringent provisions, even for registration, have been adopted in New York. This does not give as near a solution of the problem as the pure-air movement in Germany, and the attempt being made to remove consumptives from the cities into special isolated colonies. The object of this movement is: (1) To give pure healthy air as free as possible from the germs of infectious diseases to the inhabitants of cities and where the people are crowded. (2) To render the recovery of consumptives more probable by confining them to special health institutions where public spirit and government aid have provided almost ideal possibilities for the treatment of the disease. It is proposed in Germany to have air rights, as we, in this State, have water rights, thus insuring a certain quantity of fresh air to every inhabitant. Vaughan estimates from the United States census of 1890, that the total number of persons in this country afflicted with tuberculosis is about 1,050,000, or about 1 in every 60 of the population. In its early history California was remarkably free from consumption, as well as from all chest affections, owing to the great salubrity of the climate, but this very salubrity has attracted large numbers of persons from other States who were suffering from the disease, so that the mortality is now much increased. At the present time the mortality of the State seems to be smaller than in the period from 1870 to 1884. Consumption cannot possibly make any headway in California if we observe the intelligent rules laid down by Cornet. The greatest enemy of the tubercle bacillus is sunshine, and the daily average of sunshine in California is hardly exceeded by any other place in the world. If, therefore, we remove our consumptives from cities, towns, and settlements and give them plenty of sunshine and pure air, with an open air life, we offer them the best known means of improving their condition. The speaker closed his paper with a review, illustrated by figures, of the movements undertaken in Germany, France, Austria, etc., for the segregation and treatment of tuberculosis. He said: "It has been my endeavor to bring before you facts about the prevalence of tuberculosis in our midst, and the alarming increase of this disease, especially in Southern California, and to demonstrate a practical measure of restoring those afflicted with tuberculosis to health, and at the same time to protect the community at large from the danger of infection."

Dr. R. F. Rooney, of Auburn: I am thoroughly and completely in accord with the views of Dr. Richter on this very important subject. There is no cure for tuberculosis excepting a climatic cure; and previous to establishing colonies, the climatic conditions of the various parts of the State ought to be thoroughly investigated by uninterested parties. It might, perhaps, be done under the auspices of the State Society better than any other way. All the facts concerning the different localities should be brought together and published, and then every physician could diagnose his case, individualize his patient, and send him to that portion of the State best fitted for him. As Dr. Solis Cohen has said, "there can no more be one climate for all than there can be one coat for all." We have the mountain air with its balsamic and terebinthinate odors, which is suitable for one class of cases. Then again we have the sea-level, which is suitable for another, and the actual ocean air, or life at sea, which is most important for others. The mountain air is best: (1) For those cases of marked heredity or predisposition, in which phthisis is either threatened or in the early stages of development. (2) For imperfect thoracic and pulmonary development. (3) For hemorrhagic phthisis. (4) For chronic pneumonia, without bronchiectasis, which does not resolve. (5) For chronic pleurisy, where the lung does not expand after removal or absorption of fluid. (6) For phthisis accompanied by more or less pneumonic consolidation. (7) For chronic tubercular phthisis in its various stages, provided the lung surface be not too largely involved to admit of proper respiratory changes at a high altitude and there be no pyrexia. Mountain climates are contraindicated in phthisis complicated with emphysema with bronchiectasis; in cases where the heart and great vessels are affected; in complications, as diseases of the spinal cord and brain, where there is great hypersensibility of the nervous system. In diseases of the liver and kidneys, and in diabetes; in catarrhal phthisis, and in phthisis with double cavities, with or without pyrexia; in cases in which the pulmonary area is too largely encroached upon to permit of proper performance of the respiratory functions, in cavity cases, especially double cavity cases, with profuse hemorrhages, and in cases of advanced years where the patient is too feeble to take exercise. Sea voyages are best suited to cases of hemorrhagic phthisis and scrofulous cases; cases complicated with emphysema, and cases of limited disease in either mentally or bodily overworked patients. In these latter, probably the desert air of Indio, below sea-level, would be very beneficial. These things ought to be studied before any colonies are established. Then, if the various parts of the State are differentiated in this way, we can locate our patients intelligently and can prescribe the climate for a particular case just as accurately as we can prescribe any other medicine. Unless the different portions of the State are differentiated in that way, we can go on prescribing climate in an irregular and haphazard way, with the liability of injuring our patients instead of doing them good, and without any reason for the hope that is in us.

AFTERNOON SESSION.

Committee on Diseases of the Mind and Nervous Diseases.—A. W. HOISHOLT, of Stockton, Chairman of the Committee, read a report entitled, "Some Statistical Observations on the subject of the Etiology and Symptomatology of Dementia Paralytica." After giving a few facts connected with the history of the disease from its first notice by Willis, in 1672, to the publication of Prof. Mendel's monograph, "Die Progressive Paralyse der Irren," in 1880, and stating the modern views as to the pathological anatomy of general paralysis, the author gave the results of his study of 69 male paralytics, who had been inmates of the Stockton asylum between April 1, 1890, and April 1, 1897. Of these, 50 had died, 19 were still inmates. During the first five years 42 had been received, of whom 2 only were still living. Of the remaining 27 cases, 10 had died. The average percentage of paralytics to all cases (1,342) of insanity admitted (excluding readmissions) was 5.13 per cent., which is low compared with the reports from European asylums, and is probably due to the difference in the character of the cases admitted, which in the Stockton asylum would tend to increase the number of admissions without adding correspondingly to the number of general paralytics. The proportion of female to male paralytics in the State asylum was 1.35 per cent. to 5.13 per cent., or 1 to 3.8, which corroborates reports from most asylums. As regards nativity, 33 were Americans, 36 foreigners; of the latter, 1 was Chinese, a percentage of 1.09 of the total first admissions of Chinese (91), which would seem to show that general paralysis is much more frequent among white people than among Chinese. Of the 69 paralytics, 6 were convicts, while only 36 convicts were admitted during the same period, which would give a percentage of paralytics 3.45 times greater than that found among paralytics who were not convicts. 56.25 per cent. of the 69 cases occurred in individuals between the ages of 35 and 44, the age of the greatest bodily and mental development. On the subject of etiology the author gives a resume of the modern ideas as to the relation of syphilis to general paralysis, regretting that the information on this point obtainable in this and other States of the Union was of but little value. There was a history of syphilis in his cases in 37.68 per cent. He found heredity present in 10.14 per cent., while in insanity in general there is a history of heredity in one-third of the cases. Trauma was mentioned as an occasional cause and a case was given in illustration. There was a history of a prior attack of insanity in 2 of the 69 cases, which, however, might have been instances of remission of the disease. The average duration of asylum treatment in the 50 deceased cases was 16.74 months, which is almost exactly the same as that reported from asylums at Berlin and Hamburg in 1404 cases. This would seem to show that climatic influences and treatment have but little effect upon the course of the disease. He had examined into the question of the presence of ulnar analgesia in general paralytics, and the results of his investigation corroborate those of Cramer, Hess, Göbel, and others. He found unilateral or bilateral analgesia present in

85.7 per cent. of 21 paralytics, in 38 per cent. of 21 epileptics, and in 23.8 per cent. of 21 other cases of insanity. The author then took up the mental symptoms met with in general paralysis, dwelling particularly upon hallucinations and attacks of fear, giving a clinical history of 2 cases in illustration. In conclusion, he spoke of the importance of the differential diagnosis of dementia paralytica in its early stage, especially from cerebral neurasthenia.

Neurasthenia.—H. N. RUCKER, of Oakland, read a paper upon this subject, which he said would deal with the special features of this ailment. Neurasthenia in early life, that is, in school children, is more sudden in its onset and more transient than in adult life. It is an almost unfailing indication of inherited nervous instability. Such children are active, ambitious, precocious, without the physical stamina to enable them to compete with their fellows. After the inevitable break down, if the medical attendant appreciates the situation, he does the best possible thing by advising that the child shall be kept from school for an extended period and provided with wholesome exercise. The speaker believed that cases of this character, especially in girls, had, heretofore, not been well understood. Another, and far larger class of neurasthenics are to be found in both sexes between the ages of 20 and 40 years. The struggle for life during this period was at its height, hence there is a greater tax upon the nerve forces. Undue application or mental overwork, under improper conditions, will usually show their effect sooner or later in those having a predisposition to nervous instability. Rest and change from the ordinary habits of life will, as a rule, succeed in restoring man's lessened vitality. With females the result is more uncertain, as many of the consequences of nervous exhaustion tend to become permanent. Efforts made for the relief of such patients have given rise to many abuses in abdominal surgery. A third class of neurasthenics may be found in those who have passed the meridian of life, and have gone beyond the border line of final recovery, who, weighted with responsibilities too heavy to bear, then become prematurely old. Despondency ending in suicide is sometimes the result of this condition. In conclusion, the speaker said, neurasthenia, like hysteria, is an ailment of the protean type, and there is scarcely a disease which it does not, in some respect, resemble. In the treatment, rest is the all important factor, coupled with recreation in a moderate degree.

DR. A. W. HOISHOLT, of Stockton: I have listened with great interest to the paper of Dr. Rucker, and I think the subject is one that cannot be brought to the attention of the profession and of the public often enough. The hygiene, which the doctor briefly referred to, is of the greatest importance, and as the disease of neurasthenia has been termed the disease of the age, which it is, and as it is becoming more and more frequent, the proper understanding of it is important. I have seen cases in children which began as neurasthenia. The child was overtaxed at school, and, having great ambition, was unable to accomplish what it desired. The result was that doubt arose in its mind; this doubt became morbid, and finally the child doubted its ability to do anything, and became apprehensive of almost everything. Such cases become juvenile insanity. In this connection, I will again refer to what I said in my paper a short time ago, that you have in general paralysis in the early stage what has been called the neurasthenic stage of general paralysis, in which you can have all the symptoms that the author has enumerated. The treatment that has been instituted very recently, namely, the anti-syphilitic treatment, in the early stage of general paralysis, has by some late investigators been found to have marked results on the course of the disease—they report cures. If that be the case, the important thing is to recognize that disease early, and to bear it in mind in coming across cases of neurasthenia.

Committee on Surgery.—The report of this committee [published at p. 240], was read by the Chairman, T. W. HUNTINGTON, of Sacramento, the title being "The Progress of Surgery During the Last Decade."

DR. D. D. CROWLEY, of Oakland: We all have some little ideas that differ from those of others upon surgery, and one of the ideas that the author expressed I shall differ from slightly. I do not think it is absolutely necessary to close the abdomen by different layers. I think that one row of sutures is sufficient. In several cases I have experienced no hernia as the result of closing the abdomen by one row of sutures. The author also mentioned the use of catgut. I think it is a most unreliable suture. It is frequently very friable, frequently breaks when you try to tie; of late I have left it alone and have resorted to silk entirely. Regarding brain surgery, I do not think that an operation upon the skull for epilepsy, trephining, is of any more value than operating upon the femur. I think in either operation the mere fact that we are keeping the patient quiet and rectifying the diet will, possibly, for a few months benefit him. I have operated twice for epilepsy, and for two months in each case the patient seemed to recover, but after the second month he was as bad as before. I have also operated twice for loss of speech, over the fissure of Rolando, I think in both instances, one without any trauma, and the second with trauma. They were both benefited; the speech returned in both cases. A very interesting point in one case was that where, after trephining, I closed the wound with gauze, granulations formed thereafter and pus, and the patient after five days completely lost her speech, first losing part of a sentence, and after awhile getting worse, until on the fifth day she could not repeat a single word. After curetting the wound and removing the granulations and pus, the speech returned in the same manner in which it was lost. In a case recently of a man who had had a fracture of the skull, he also lost his speech, but on trephining the speech returned, and he can now speak as well as ever. In regard to the vermiform appendix, there is only one point that the author failed to dwell upon. It is generally supposed that a foreign body must be present in the vermiform appendix where there is inflammation of that part. I have as yet found no foreign body except fecal matter. In cases of ulceration of the intestines, from whatever cause, I think it is the duty of the surgeon to cut down, locate the ulceration, remove the ulcer, and stitch up the intestine. A very excellent method of discovering a perforation is by the injection of gas in the rectum; by forcing the gas into the rectum it will escape from any perforation, and if you pour sterilized water into the abdominal wound over the intestine it will cause a bubbling, and you can easily discover the wound. [*Vide* MEDICAL TIMES, vol. vi, p. 24.] Regarding stab wounds of the abdomen, I believe it is the duty of the surgeon in every instance to make the wound larger and to explore. As to anastomosis of the intestines, I think, as Dr. Huntington has said, that there is nothing that will take the place of the Murphy button. The rapidity with which the operation can be performed is a great point in its favor. I believe that in 15 minutes after the abdomen is opened you can easily place those buttons in position and connect them, and then you have the intestine united. There is only one fault that the surgeon often commits in using the Murphy button, and that is he uses a button too large for the intestine. This produces a pressure from within upon the wall of the intestine sufficient sometimes to cause an ulceration around the button, and a perforation will take place therefrom. I have committed the same fault in operating upon a dog. In pyloric stenosis, I think that if there is disease of that part by making an incision through the contracted portion, both in front and behind, and connecting the healthy parts, you can then make a transverse suture both in front and behind, and you have one part of the pyloric end of the stomach in connection with another part without removing anything. I believe that same operation has been performed upon the ureter.

DR. GEORGE L. COLE, of Los Angeles: In regard to closing the abdominal walls, I would like to call attention to a little maneuver which I saw Dr. Murphy, of Chicago, perform a few weeks ago. Instead of closing the walls in separate layers, he did what practically amounted to it. In using the silkworm gut he carried it downward through the cutaneous structures and the subcutaneous tissues on one side, and then carried the same suture across to the opposite side, but through the deeper layer of tissues and the peritoneum, and then brought it back to the side on which he began and carried it upward through the peritoneum and a portion of the tissues, meeting the layer to which he had come down on the first penetration, and then he crossed to the opposite side up to the skin, making a figure of 8 suture. The fact would impress one that it was a very difficult suture to remove, but Dr. Murphy said he had no difficulty in removing it at the proper time. He called it the figure of 8 suture; he did not claim it as his invention. I have no doubt that Dr. Huntington knows who is the author of it, and he probably is familiar with it, but it impressed me as an ingenious suture.

Dr. G. F. Shiels, of San Francisco: The tendency on the part of the general profession is to operate altogether too much. Men who do not know anything about surgery call themselves physicians and surgeons. Whenever a paper of this kind is read it always gives a new whip-up to the fleeting horse of surgical experiment on the part of the profession at large, and a marked increase in the mortality and discomfort of the human race results. This paper certainly was most interesting to the surgeon, but I trust that it will not be a stimulus to that class of men whom we have so well represented in our midst. There are several points of more than ordinary interest that the doctor has called attention to, and one of them was the subject of operations for epilepsy, which was touched upon by Dr. Crowley. I do not think that operations for epilepsy should be performed, on general epilepsy any way, on very few cases of focal epilepsy, if we take the results of past experience as a guide. I had occasion, some few years ago, to call the attention of this Society to the fact that I had discovered a new method of curing epilepsy by operation upon the cortex, taking up large portions of bone and pricking the brain underneath, or rather in and about the fissure, with the object of creating a counter irritation, on the theory that the disease was due to explosive irritation of the cortex. The result of my operations was excellent for a time; most of them were worse than ever, after they began to have their epilepsy again. Dr. Crowley is entirely right. You can take almost any epileptic and make an incision through the tissues, and you will cure him for the time being. In relation to closing the abdominal wound, I do not know that it is really a matter of very much importance how many sutures you employ, so long as you bring the peritoneum together. It is where there is a failure in bringing the peritoneum together that you are liable to have hernia. I think that is the whole principle of the radical cure of hernia, to get the peritoneum together high up and well. I recently came across a maneuver which seemed to me to be a very good one if you wish to add to the prevention of hernia. The originator of the method I do not remember, but he cuts down on the median line until he gets to the peritoneum; then draws the muscles and structures aside and opens the peritoneum at a point distant from the median line, so that when the tissues are drawn back again into position they cover over the wound in the peritoneum, which prevents the possible occurrence of hernia. I think the author has gone a little far in thinking that all cases require operation. I understood him to say that he thought the surgeon should cut down in all cases of appendicitis. We know perfectly well that diseases in all hollow viscera may be due to causes outside of the wall, and there are many cases of inflammation of the vermiform appendix which are purely of a catarrhal nature and do not call for operative procedure at all. The patients get better and well, and there is no necessity for operation. However, I entirely agree with him that where a surgeon has charge of the case and understands what he is doing, I think that when he has thoroughly satisfied himself of the existence of appendicitis, surgical appendicitis, if I may use the term, he would always be justified in cutting down and treating the case on a broad surgical basis. I do not believe the operation should be done as universally as it is being done by every class of medical men. The whole proposition is this: take a point midway between the spine and the umbilicus and place the point of your finger upon this point and press. Pain, operate at once; no pain, patient escapes with his life. That is the proposition. Immediately, now-a-days, if a man puts his finger there and presses and gets pain, he wants to cut down, simply because it is McBurney's point, and it is to McBurney that we owe this frightful storm of operating which has gone over the whole country from east to west and from north to south. There is not a village in the United States where some doctor is not performing repeated operations for appendicitis which does not exist. Regarding excision of the rectum, when it is done for malignant cancer and is successful from the surgeon's point of view, he gets into a condition of mental admiration and admires his work and says, "What a wonderful thing I have done; I have taken off two inches of the rectum;" but the patient dies in the large majority of cases. The idea that you can put your forefinger into the rectum and feel a healthy bowel beyond is not a justifiable reason for thinking you can cure the cancer of the rectum, and by far the larger number of cases of cancer recur and the patient dies, and I am very much more in favor of performing inguinal colotomy and permitting the cancer to cause as little discomfort as possible to the patient until eventually it kills him as it nearly always does. Excision of the rectum for fibrous growths is another proposition altogether, and should, I think, be performed. In regard to the operation for perforation of the bowel by typhoid ulcer, I might mention a point which would interest the surgeons here. Some years ago, on the invitation of Dr. Wooster, since dead, I performed an operation for appendicitis on a young fellow, and the case was supposed to be one of typhoid fever. We removed the appendix, which had no particular appearance of disease. The boy went on and went through his typhoid fever, perfectly well, showing that you can operate on the bowel in typhoid fever without any especial danger to the patient. In regard to operations for hernia in the aged: If you have a man advanced in years who has a hernia which is easily retainable by a truss, and does not give him any particular bodily discomfort, I believe you should not operate. I do not know of better cases to operate on than the very young. They recover well and the operation is a great success. I have operated on quite a number of cases of hernia. I believe if you wish to get an absolutely radical cure you had better remove the testicle in the male on that side, so that you get rid of this cord which makes the difficulty in closing the opening. If you take that testicle away, one can carry out the function of both. If you do remove the testicle on that side you will find it is easy to cure your patient absolutely.

Dr. E. A. Follansbee, of Los Angeles: I would like to thank Dr. Huntington for his paper on behalf of the general practitioners here assembled. I do not think that any of us who have listened to this paper will be disposed to do any rash surgery as a result of it One of the great advantages of the State Society to me has always been that I have been enabled to listen to papers from those who are authorities in certain specialties that are not mine, and that I felt that my general education was being improved; and I think it would be a great deprivation to our Society if papers like this should be relegated to sections where only those who are expert operators could hear them. I think we all enjoy them, and, contrary to the expectation of Dr. Shiels, I do not think that there will be any serious results from having heard this one.

Dr. J H. Barbat, of San Francisco: I did not have the pleasure of hearing the first part of Dr. Huntington's paper relating to closing the abdomen. I judge, from the criticism made, that he gave three layers of sutures as the proper method of closing the abdomen, and I was rather surprised to hear Dr. Shiels remark that the closure of the peritoneum was the principal thing that was going to prevent hernia. We know very well that the peritoneum plays a very small part in the prevention of hernia. It is the close apposition of the deep fasciæ, derived from the muscles of the abdomen, which prevents hernia. It is proper to close the peritoneum, undoubtedly, but the principal portion of the closure of the abdomen consists in the close apposition of the fibers derived from the oblique muscles. The peritoneum and skin are almost secondary to this, while I believe it is proper to begin the closure with a piece of fine catgut in the peritoneum. Dr. Crowley made the statement that catgut is going out of use. I have been using catgut right along, and my impression is that when catgut is used, and we have sepsis, it is not the fault of the catgut, but the fault of the operator or the nurse; it has been dragged over something and rendered septic. Dr. Crowley says he has never found anything in the appendix but fecal matter. I had not until yesterday, when in assisting Dr. Carpenter in an operation in which we removed the appendix, we were rather disappointed to find it looking quite decent when we opened the abdomen, as we had gone in after it. We took it out, however, and on opening it we found gooseberry seeds. This is the first instance in which I have ever seen anything but concretions of fecal matter. I have seen concretions of fecal matter which resembled seeds, and one which resembled a grape seed, and on cutting it open I found it nothing but a concretion of fecal matter. I think in the majority of cases in which surgeons have maintained that they have found seeds they have been concretions of fecal matter, resembling seeds from fruit. In regard to operations on the gall bladder, I certainly favor the most conservative method. In my opinion the operation for obstruction to either the cystic or the common duct consists in making an anastomosis between the gall bladder and the intestine, preferably the descending portion of the duodenum If the gall bladder is removed we have no means left in case any duct becomes obstructed, and the patient is in a bad fix. If we make an anastomosis between the gall bladder and the duodenum, the patient goes on and we have no further trouble whatever. If the common duct becomes obstructed we have an artificial duct, which takes its place very well. I have operated on a few cases with good results, and I have not heard any histories of bad results. There have been a great many theories put forth as to the possibilities of food getting into the gall bladder, and of infection passing up from the bowel into the gall bladder and producing catarrhal conditions. This has not been the history of these cases.

Dr. J. C. Stinson, of San Francisco: I cannot agree with Dr. Barbat that infection of the catgut is usually from dragging it over a basin, or something like that. I think the infection is usually due to imperfect preparation. As regards appendicitis, I think we should not take any steps towards operating until we are sure of our diagnosis, and, then, being sure, we should go to work at once. In regard to excision of the rectum, if we can get a case of cancer early enough, before it has involved the bladder or adjacent tissues, I think we should excise; otherwise, in a case where there is no obstruction we should let it alone. If there is obstruction, there should be excision. In closing the abdominal wound I think the idea is, in curing hernia, that each and every particular structure should receive its share of attention. In closing the peritoneum we should seek to bring the edges accurately together, and in closing the other layers each should be brought accurately together with catgut sutures. Some time ago I wrote a paper reporting cases operated on by this method, and I think that in every instance, where primary union is obtained and catgut used, every case can be cured.

Dr. Huntington, of Sacramento: I have been taken to task—perhaps properly—for reading the kind of paper I did before a mixed audience. I believe I was not taken to task upon the proper ground, however, because I do not think there is any danger before a mixed audience of inducing men to do rash things. If there was any danger at all it was from the fact that men who are not dealing with work on these lines would not be particularly interested in this, and the paper had been better relegated to a section to people who are particularly interested in it. With regard to the point made by Dr. Shiels, I want to say this, and this is about all: I want to insist upon a point which I made in the close of the paper with regard to the great difficulties that hang over the work of the surgeon to-day, not in avoiding the risk of incurring mortality, but in the great dangers there are in getting the kind of result which the patient and his friends expect, and which the surgeon is too prone to lead them to expect. Senns' remark, to me, was a very significant one, and I shall keep that in mind as long as I use the knife, for I believe that Senns' experience will be repeated not only next week, but next year and for all time in the history of surgery. I never expect to see the time when we shall come any nearer perfection in the treatment of wounds than we have attained to-day, excepting that we perfect some little detail which may remove one more danger. We shall always have suppuration where we do not expect it, and the operator, I do not care who he be or where he operates or what is his environment, will always see pus before he closes his day's work, if he tells the truth. There has been forgetfulness and carelessness of observation; there has been, I will not say untruthfulness, but there has been negligence in the recording of results along these lines. For a period of years I thought that it was possible that the words of Sir Joseph Lister, upon a notable occasion, could be verified, and that the knee-joint could be opened without any possible risk to the patient. I have long since modified my views in that connection, and I believe to-day that while the careful surgeon, with perfect conditions as they can be had, will open joints or abdominal cavities successfully in a large number of cases, he now and then will run against a snag where he least expects it. I think the warning of these men who are to-day doing a large amount of work cannot be raised too positively or too often upon this point.

The Radical Cure of Hernia with a Report of Cases.—W. LeMoyne Wills, of Los Angeles, read a paper upon this subject. He said that much confusion had arisen in the profession owing to the multiplicity of operations to which names have been attached, yet which were very similar in principle, many being practically identical. He then gave an illustration of the various operations under the names of the operators, pointing out the differences when any existed. In a similar manner the herniæ were discussed and differentiated. He advocated bolder methods than had been customary and laid great stress upon the necessity for absolute asepsis in these cases. Regarding methods of procedure, he expressed his preference for the plans recommended by Bazini and Halstead.

Dr. C. G. Levinson, of San Francisco: Before beginning this discussion I would like to put a question to the Society with regard to the sterilization of kangaroo tendon, upon which Dr. Wills has not laid stress, but which, to my mind, is one of the most important things that we must look after in operations for hernia. We have many cases of suppuration which we are apt to attribute to necrosis of fat, etc., which are due to the pressure of the suture. We seem to forget that the kangaroo tendon, which is in general use at the present time, might have a great deal to do with the bad results that we suffer from. I think the general run of men use kangaroo tendon which is already prepared and that must of necessity be imperfect at times. I have had some experience in the sterilization of kangaroo tendon according to the method of Marcy. I think that is the latest method that has been advocated for the sterilization of kangaroo tendon in hot linseed oil. We must realize that the size of the kangaroo tendon used for hernia operations is larger than the caliber of any catgut suture that we use, and, as a consequence, is the most difficult to sterilize. I would like to hear the experience of men who have had some experience in the sterilization of kangaroo tendon, as to their methods and success.

Dr. T. W. Huntington, of Sacramento: I rise to correct one statement made by the writer with regard to the use of catgut in hernia operations by myself. Dr. Wills was certainly mistaken in my statement. I said to him that I was preparing to use it; that Dr. Rockey, of Portland, Or , had recommended it very highly and regards it as a most favorable suture material. In regard to the sterilization of kangaroo tendon, I have used for two years, perhaps, the kangaroo tendon of Marcy, and I have used it with ordinarily good results. Something disappoints me now and then, and here is a point bearing upon this very subject. In a recent case of double hernia, where the conditions on either side were practically the same, where the environment and *technique* and everything else was all the same, the right side suppurated and the left did not. I would like to know why that is. It is a subject which I have studied very deeply and I cannot account for it by any neglect. There is a secret in all this that we have not discovered. The third layer of the skin does now and then contain the germs of suppuration of all kinds. I shall certainly not attempt to do anything more with kangaroo tendon than accept it as Marcy sends it. I apply to him directly for it and he sends it to me, assuring me that it is in sealed packages and properly prepared. I believe this to be true.

Dr. C. Ford, of San Francisco: I think the general practitioners here who are not very well up on surgery, and not particularly expert in recognizing the different operations by the different names, will be greatly mixed up; far more mixed up than they could be by anything that was not said. I think that is a most remarkable classification for the different operations of hernia. It is so remarkable that it will not bear criticism at all. So I will pass that point entirely, excepting to say that the recognized distinction between the different operations now-a-days, is not so much in the way of treating the sac as in the way of treating the cord. I was very sorry to perceive that he had not mentioned Dr. Stinson, who is doing some very excellent work in that line and who has described an operation which I have done several times with the most beautiful and perfect success: that is, the non-transplantation of the cord with methods of his own, which he mentioned in a previous discussion. The most important thing is coaptation of the tissues. That has been recognized by every operator for hernia. This has given rise to inventions or methods, rather, of suturing the different tissues by the different men. The stitch of Halstead is called a mattress suture. It is merely the interrupted suture placed inside. The stitch of Marcy is the cobbler's stitch. The stitch of operators vary. I have recently invented and described a stitch with which, perhaps, some of you are familiar. It amounts to a rapid and continuous insertion of the interrupted suture.

Dr. G. F. Shiels, of San Francisco: There seems to be, all over the world, a sort of idea that you must have for sutures substances which will last a long time in wounds, to bring them together. All that is necessary in any wound is to use a material which will act as a splint in keeping the tissues sufficiently long together for them to heal naturally. After that they act as foreign bodies. Therefore the idea that it is necessary to use any material which will last for any great length of time seems to be ridiculous. All the suture does is to bring the tissues into position and to hold them there until they have healed. After that the suture ceases to have any function; it is absorbed and done away with and has no function.

Dr. G. L. Cole, of Los Angeles: In regard to the cases that Dr. Wills operated on, I wish to say that I have seen several of them, and saw them during the operation, and it is not surprising that he should find pus in some of them. Those that I saw him operate on were in the County Hospital, and he was very careful to demonstrate the operation from the beginning to the end to his whole class of students. The case he referred to as having returned from Arizona was one of the most beautiful class demonstrations of hernia that I have ever seen.

Dr. J. C. Stinson, of San Francisco: I think the principal thing with which the operator should be familiar is the anatomy of the parts. The author did not go into this very thoroughly. I may say the anatomy of inguinal hernia is very imperfectly understood at the present time. The rings are intimately blended with the structure which passes through it. We cannot speak of the canal as having an anterior wall and a posterior wall, and an upper floor and a lower floor, because these terms all tend to confuse. Practically, we must think of the canal as being formed only by the roof and the floor and by an external and internal ring placed completely in the groins and containing the cord which passes along towards the pubic bone. I think if we will look at it in this light, and as it is, we will know how to attack our hernia, and how to cure it. I would not cut down boldly as the author says, because I think I would get into trouble; I would cut down very cautiously, and try to do things carefully. I never saw an operator cut down boldly as the doctor does; in fact, I do not think it wise. The truss, I think, should be used in children and old people. If a person is in good general condition, I think an operation can be done without any bad effects. I think the method of anchoring the sac, of which the doctor spoke, is wrong. It should not be anchored, because it acts as a wedge; and, being acted on by the intestines, will only serve to open the canal. As Dr. Shiels said, the cord and testicle are the important features to be considered. If the internal ring is closed and the canal reinforced and the external wound made small, allowing room for the cord, with a perfect suture, and placing the cord next to the pubic bone, there is no reason why a man operated on by this method should have hernia any more than an individual who has a weak spot in that area. The author spoke of Coley using catgut. I will correct him, and say that Coley does not use catgut, and has not used it for four or five years. He uses the chromacized tendon. I think it is wrong for an operator to remove all healthy membrane, because internal hemorrhage may occur, and the patient die from that cause or from peritonitis. As regards kangaroo tendons, I think the chromacized tendons, as prepared by Marcy, are excellent. I cannot remember one case out of some 86 cases, including some 8 cases I have had since I have been here, where I can say that suppuration was due to the chromacized tendon. I think preserving the chromacized tendon in alcohol is better. The relapse in cases, I think, is due to the wound not healing properly, and to imperfect suture and imperfect suture material. I think if the wound is treated by sterilized tendon, and each of the layers sutured separately, with the cord next to the pubic bone, relapses will not occur. I prefer for my part the healing by primary union to secondary scar.

Dr. Wills, of Los Angeles: I would like a few moments to answer some of the hypercriticisms. In the first place, as to the sterilization of kangaroo tendon, I would prefer to trust to a laboratory than to trust myself, or a general practitioner, or a general surgeon. I do not believe that many physicians or surgeons, no matter how good they are, can do better than an honest laboratory that wants to protect its reputation and make money. I plead guilty to using commercial tendon. I put it in 1:1000 solution of bichloride of mercury.

Whether that does any good or not I do not know. I would rather trust to a laboratory, or to any of these men who put out first-class ligatures and surgical goods, than to do it myself. In regard to Dr. Ford's criticism that that was the most remarkable classification he had ever heard, I have no doubt it is, and it is entirely original with me, and I will stand good for it. As I said, I wrote this paper for the general practitioner and not for the hernia specialist. I do not pose as one. I do not come here to say "I," and big "I" every few minutes; I came here to evoke discussion. I made that classification at random, but I will say it is a good one, and I will back it up by authorities. It is an attempt to simplify and to make it possible for the average physician without much reading or looking up authorities to understand hernia, and the classification of writers. They differ as to the details, but the object is the same, whether you have the cord transplanted or retained in the inguinal canal. If you transplant the cord you have a second class. If you use the McEwen or the double method, you have a buttress and pad. As to the remark of Dr. Stinson about boldly cutting down on the tissues, if a man knows his anatomy he can cut boldly where another man dare not go. If a man does not understand anatomy he has no business to try to do hernia; and if he does not know his anatomy he has no right to try to do any kind of surgery. I claim to know the anatomy of the inguinal region, and I do cut boldly. I have not cut anv deep epigastric arteries, and I have operated right over the top of the iliac artery and the femoral artery. It is because a man is sure of himself that he can do that. As to the criticism that Coley does not use catgut, I beg to differ, and to say that he does use it. As late as 1893 I had the pleasure of seeing a good deal of his work.

Serum Therapy in Surgery.—G. B. SOMERS, of San Francisco, read a paper upon this subject. He said: The phenomenon of immunity is now beginning to be understood, and is the goal of the newer therapeutics. In the light of our present knowledge of disease, the following propositions have received general acceptance: (1) The causes of a majority of diseases are toxines produced by microörganisms. (2) Blood serum may acquire an antitoxine in the presence of toxines or microörganisms. (3) Both toxines and antitoxines may be separated from the blood by chemical means. (4) Each toxine varies according to the toxine it is designed to neutralize. (5) When present, antitoxine not only protects the animal in which it is found, but also protects or cures other animals when drawn and injected into them. These propositions are the basis of serum therapy. As it becomes perfected in its details it seems destined to revolutionize the practice of medicine and surgery, for it promises to transfer many affections now recognized as surgical, to the province of medicine. The special feature of antitoxine is that though strongly antagonistic to germs and their products, it has no harmful effect on tissue-cells. Antitoxine is nature's antiseptic. The animal cell produces it as a means of self-defence in the struggle for existence. In the higher orders of animals the production of the various antitoxines is carried out through a division of labor among the cells, and it is here that we shall probably find an explanation of the function of the various ductless glands whose use has hitherto remained unknown. The diseases most interesting to the surgeon are those due to the streptococci and staphylococci, but as more work has been done in investigating the streptococci than any of the other pus producing germs, it will be convenient and quite sufficient for purposes of illustration, to examine what has been done in the diseases due to this germ alone. Several difficulties arise in attempts to produce an antistreptococci serum, the chief being that the distinction between the varieties of streptococci are not well made out. In order to be accurate in the application of serum therapy against streptococci, we must first determine the exact germ and then find a corresponding antitoxine. Practically, antistreptococci serum has been used with favorable results in erysipelas, septicemia, puerperal fever, phlegmon, scarlet fever, the suppurative complications of tuberculosis, and many other conditions accompanied by deep-seated suppuration. Some remarkable examples of cures in the above diseases have been reported. It is desirable to attract attention to the subject, for wide spread interest will aid in perfecting it, and if it fulfills its promise of providing a remedy but for septicemia alone, it will be an invaluable boon to the human race.

Neuropathic Gangrene of the Skin.—Emmet Rixford, of San Francisco, read a paper entitled "A Case of Neuropathic Gangrene of the Skin." A young woman, 18 years of age, presented herself at Cooper College Clinic early in January, exhibiting extensive gangrene of the skin of the right hand and arm. Six months previously she had burned the radial side of the wrist with strong potash lye, producing a sore which was very slow in healing. From time to time small sores formed near by. On January 1st a large area on the back of the hand became gangrenous. From this time on patches of skin ½-inch to 1½ inches in diameter would die every few days, till the greater part of the volar surface of the forearm, arm, and deltoid region was involved, and a few isolated spots formed on the neck and breast. The gangrene is limited to the skin and superficial fascia; it is dry, contracted, and sunken, with sharply defined edges, and the slough is thrown off after a few days, leaving a healthy ulcer with sharp, but not undermined edges, the floor covered with bright red granulations which speedily fill up the cavity. The cicatrices are generally keloidal. The patient is well nourished, weighs in the neighborhood of 150 pounds, is strong and vigorous. She walks every day from her home, south of Market street, to Cooper College and back, a distance of at least four miles. There is absolutely no evidence of syphilis; the iodide, 20 grs., three times a day for a month made no impression on the progress of the lesions. There is complete anesthesia to touch, heat, and pain over a very large area, including the volar surface of forearm and arm, the deltoid region, the neck as high as the lobe of the ear, the chest as far down as the nipple. Raynaud's disease is excluded by the want of symmetry in the lesions, the absence of local asphyxia and local syncope. Diabetes is excluded by the continued absence of sugar from the urine. The disease is too rapid for leprosy; there are no nodules on the ulnar nerve, though it is anesthetic; there are no other patches of anesthesia on the body. Syringomyelia would almost certainly produce lesions on the other side as well, and would not cause interference with the special senses, as occurs in this case. There is no scoliosis, no muscular atrophy demonstrated; measurements of both arms are alike. The patient is severely hysterical. Besides the local anesthesia there is loss of taste on the right side of the tongue, loss of smell and hearing on the same side, and centrally contracted field of vision. The flexors of the three smaller fingers are held in tonic spasm. There is, however, no history of globus, and pressure over the ovaries fails to produce it. Most cases of skin diseases in hysterical patients are self-induced, but careful watching over a long period and close observation of the course of the lesions, make it practically certain that this is not the case here. The case is, then, one of extensive spontaneous gangrene of the skin due to hysteria. The burn with lye furnished the suggestion which directed the patient's mind to the right arm strongly enough to cause vasomotor disturbances sufficient to cause death of the skin. No treatment thus far tried has had the slightest restraining power on the gangrene.

Laminectomy Six Months after Fracture—Recovery.—O. J. Mayer, of San Francisco, read a paper upon this subject. The patient, a man aged 27 years, was, on April 23, 1894, engaged in bringing a freight elevator from a cellar to the sidewalk, the trap doors in which were closed. When five feet from the sidewalk he found that the elevator could not be stopped, and, after several futile attempts, he tried to slide off the platform, but was caught about the waist line and crushed between the platform of the elevator and the ceiling of the basement. He remembers hearing some bones snap and then became unconscious. He probably lay in this position for ten minutes before being discovered, and, when assistance arrived, in lowering the elevator, his body fell from the platform to the basement floor, a distance of about twelve feet. Some hours after, when he regained consciousness, he complained of pain in his back, sharper over the left lower ribs, and some pain upon moving his arm. He could not move his lower extremities and had no feeling in them. After three days he was removed to a hospital where he was placed in a plaster of Paris jacket, extending from the arm-pits to the toes, in which he remained for seven weeks. When the jacket was removed sloughs were found over the scapula, the sacro-iliac synchrondroses, over the sacrum, the heads of both fibulæ, and both heels. Four and a half months later his condition was unchanged, except that the bladder was more irritable and the sores had become enlarged. Three months later, on February 4, 1895, the patient was first seen by the speaker. There was complete paralysis of the lower extremities, with considerable atrophy. The knee, ankle, and tarsal joints were ankylotic. There was only slight movement of the hip joints. February 19th, under chloroform, the joints were manipulated—the left knee, which offered considerable resistance, finally yielded sharply, when it was found that a vertical fracture of the patella had been produced. Three weeks later, manipulation, under chloroform, was again attempted and this was performed twice later, within seven weeks. At the end of June the bladder symptoms had improved, though there was still incontinence. The patient could use his thighs so as to throw the legs forward and was able to insecurely stand and walk a few steps with the aid of crutches. On September 6, 1895. laminectomy was performed. An incision was made from the spinous process of the tenth dorsal to that of the fifth lumbar vertebræ. The muscle, fascia, and ligaments were detached close to the bone on either side of the processes, and the hemorrhage, though very profuse, was readily arrested with hot salt solution. The processes of the first and second lumbar vertebræ were movable upon manipulation. The spinous processes of the first, second, and third lumbar vertebræ were then removed close to the base. The laminæ of the first and second lumbar vertebræ showed a decided recedence from their proper position and were removed. The dura was adherent to the cord and was not opened. Careful examination up and down the canal gave no evidence of further encroachment. The wound was closed with deeply placed silkworm gut sutures. The dressing was changed on the third day, and by September 20th, the wound had healed. For the fifteen days following the operation there was no improvement of the general symptoms, but at that time the patient first asked for the vessel—the bowels moved voluntarily. Vesical incontinence still continued. On October 21st the patient left the hospital, being able to use his crutches fairly well. When seen at the speaker's office on November 27th, he was walking with the aid of one stick. The legs are used in an ataxic manner. Improvement has since continued until he can now retain urine three or four hours. He can walk several miles, and up and down stairs without support. Quite a depression exists where the processes and laminæ were removed, but firm fibrous adhesions have formed, to some extent filling in the space.

EVENING SESSION.

The Sideroscope and the large Haab Electro-Magnet.—A. BARKAN, of San Francisco, exhibited both these instruments. He explained their mode of operation at length, and with the aid of assistants demonstrated their uses.

Demonstration of X-Ray Apparatus, and its Uses in Diagnosis.—PHILIP MILLS JONES, of San Francisco, read a paper in connection with the subject of diagnosis by the X-rays. In this, the author, who has had a great deal of personal experience, outlined at length the origin of the discovery and its gradual development and perfection. He gave a number of cases illustrative of its benefits, and exhibited a series of skiagraphs in illustration. The apparatus, in which the static machine was used to produce the spark, was next exhibited. The author called attention to the fact that the fitness of the tubes for working purposes varied from time to time, and that this variation did not depend absolutely upon the character or degree of vacuum, but that there seemed to be some other factor involved. He alluded briefly to the unpleasant consequences sometimes following the use of the X-ray. He believed that these local lesions were generally the result of over exposure, though not denying that they were possible in highly sensitive individuals.

Demonstration of the Roentgen Rays in Cardiac Diagnosis.—ALBERT ABRAMS, of San Francisco, spoke at some length on the uses of the X-rays in the diagnosis of cardiac affections involving change in the position and size of that organ. The demonstration by the speaker, which was very elaborate, included the exhibition by an assistant, by means of the oxy-hydrogen light of a large number of skiagraphs in connection with the subjects treated.

SECOND DAY—MORNING SESSION—WEDNESDAY, APRIL 21.

Committee on Obstetrics.—The report of this committee was read by the Chairman, D. B. VAN SLYCK, of Pasadena, the subject being "Puerperal

Fever." The speaker said he thought it would be more profitable to confine himself to a single subject, and one likely to elicit a free discussion, than to attempt to review the entire field of obstetrics. He said: Doubtless we are all agreed as to the etiology of puerperal sepsis, and there are few physicians who will not subscribe to the theory of surgical cleanliness for its prevention, while it cannot be denied that the practice in a large majority of obstetricians is notoriously lax and inefficient, By puerperal fever was meant any rise in temperature in the lying-in woman due to septic absorption from the genital tract. Speaking broadly, any rise of temperature after labor unless due to malaria, or some intercurrent febrile or inflammatory disease, should be attributed to sepsis. The consequences of this disease were far-reaching and disastrous. Subinvolution, endometritis, versions, pyosalpinx, and many forms of pelvic disease are more often due to sepsis, after labor or abortion, than to all other causes combined. The invalidism and life-long suffering of many child-bearing women often dates from a confinement or miscarriage, followed by more or less septic infection Among the savage and semi-barbarous races puerperal sepsis is practically unknown. This might, in part, be accounted for from the fact that the parturient canal is never invaded for the purposes of examination or assistance. No doubt, also, owing to the rude life of the savages there are qualities in the blood resistant to and destructive to bacteria. The rarity of puerperal fever in the rural districts in early years adds plausibility to this supposition. It might be argued that if these premises are correct we are putting ourselves to a great deal of needless trouble in attempting aseptic midwifery. He was inclined to think that too many doctors felt very much that way about their obstetrical cases, and, therefore, their antisepsis was really only a pretense. He believed, however, in the possibility of the source of infection being in the patient herself, in some cases, either as a specific virus or as a parturient endometritis, though this is rare. He believed that the profession, as a whole, was only slowly approaching the proper standard of excellence in midwifery, and that it is virtually where surgery was 15 years ago. To overcome this inertia, and that all might believe in clean obstetrics, had induced him to write this paper. The public is getting to be almost too wise on the subject of microbes, and the doctor who has frequent cases of puerperal fever will be blamed, whether or not he deserves it. The day is gone by when milk fever, catching cold, or a dispensation of Providence will be accepted as a sufficient reason for the fever, and the death of a woman from puerperal fever, from any cause, is as serious a calamity as can happen to a practitioner. The trouble of aseptic practice is only a bugbear, and is no more than is constantly done by the surgeon, even in minor operations. The main thing is to have a definite system, and to insist on its being carried out minutely. All clothing coming in contact with the patient should be washed clean, in the usual way, then boiled for half an hour and hung out in the sun on a boiled line As soon as dry each piece should be ironed with a very hot iron and put away at once in a pillow case. Pads should be thoroughly baked in an oven, and then placed aside until needed. At the onset of labor the rectum should be cleaned out by a warm water enema. The pudendal hairs should be clipped close. After a general soap and water bath, the body, from the waist down, and the thighs and genitals should be scrubbed with a 1:2000 bichloride solution; the patient should then be dressed in aseptic clothing and placed in an aseptic bed. The attendant should scrub his hands and arms with soap and water and rinse them with ether, and then scrub with a bichloride solution 1:1000, seeing that his finger nails are short and clean before disinfection. He should wear an aseptic gown, and not touch the patient's genitals for examination, or for any other purpose without disinfecting his hands anew. Examinations should be made as infrequently as possible. The nurse should be equally clean in her dress and person. In regard to auto-infection, the speaker believed the value of antiseptics had been greatly overrated. The question arises, shall we, for fear of these germs becoming virulent, attempt their destruction by *ante-partum* douches, or trust to the natural antagonism of their habitat. He believed the normal secretions of the cervix and vagina were inimical to and destructive of disease germs, as well as antitoxic, and should therefore not be disturbed. The douche, to actually disinfect, is entirely inadequate, as nothing short of scrubbing with soap and water, together with free antiseptic irrigation, would be of any use.

Much the same argument applied to the *post-partum* douche, which was only to be used when specifically indicated. *Post-partum* hemorrhage was a frequent predisposing cause of sepsis. It should, therefore, be carefully guarded against. A full dose of ergot should be given at the completion of the third stage. Means for the proper treatment of the severer forms of *post-partum* hemorrhage should always be at hand. The speaker then briefly reported a case of puerperal fever occurring in his practice as illustrating his treatment in this condition. He objected to the use of the curette as illogical. If used simply to clear the uterus, it is too dangerous an instrument. If used for the purpose of removing the entire endometrium, it was really throwing down the barrier nature had set up, and opening wide the door to further infection. The walls of the septic uterus were soft and unresisting. How many times perforation had taken place during curettage it would be interesting to know, but the speaker believed the accident was not rare. In conclusion, he wished to emphasize the importance of exploring the uterus as soon as there was any rise of temperature, not waiting the chill announcing that general infection had taken place.

DR. E. A. FOLLANSBEE, of Los Angeles: I feel that Dr. Van Slyck's paper is a very important one. It is a subject of more importance to me than any other. I think that the puerperal patient should be put under thorough surgical cleanliness, and thorough surgical asepsis should be carried out at every point during the period. Nothing short of that is proper.

DR. W. FITCH CHENEY, of San Francisco: Dr. Van Slyck says that on the first suspicion of infection in puerperal cases, an examination should be made of the uterus. In this I quite agree. But he says he thinks an examination could be made by the introduction of the finger into the uterus to determine its condition. With this I cannot quite agree. The difficulties in the way of this procedure are these: In the first place, it is not easy to introduce a finger into the uterus without first introducing the hand into the vagina. It is not an easy thing to introduce the hand into the vagina without an anesthetic; if you do introduce the hand into the vagina, and introduce the finger into the uterus, provided the infection has occurred, as it commonly does, within a few days following labor, the uterus has not yet become involuted sufficiently so that your finger will reach the fundus, and the condition is not accurately conveyed to you by the finger in the uterus. Furthermore, it is not as easy to render the finger aseptic as it is to render a metallic instrument aseptic, and the danger of conveying germs to a uterus not previously infected is greater with the finger than with an instrument. I want to emphasize, as my opinion coincides thoroughly with Dr. Van Slyck's, his objection to the use of the curette in puerperal cases. This is comparatively ancient history now, and yet the point cannot be too strongly emphasized that the curette, in the beginning of puerperal fever, often does more harm than good. Nature, in her efforts to prevent the progress of infection from the endometrium through the veins or lymphatics into the general circulation, throws out a zone of round cells, as has been proved by histologists and pathologists, to oppose a barrier, as it were, to the infection of these germs. The introduction into the uterus of the curette, and the scraping out of the mucous membrane, simply destroys this barrier thrown up against the enemy, and allows the germs to make further progress into the deeper tissues through the lymphatics of the veins and into the walls of the uterus and general circulation. This is not new, but it cannot be too strongly emphasized and repeated, and too much emphasis cannot be laid on the harm done by the use of the curette. The simple use of an intra uterine douche in the beginning is all that should ever be done. If this fails to reduce the temperature and to check the progress of the case, evidence is afforded that infection has already progressed through the uterine wall into the general circulation and then harm cannot come from thorough removal of the disease; but previous to that evidence the douche is all that is required.

DR. D. A. HODGHEAD, of San Francisco: I think that the author is on the right track in nearly every direction; but I must differ from him in one or two particulars. I notice he claims that puerperal fever is not common among the barbarous tribes, including, I suppose, our American Indians. I know that it has long been a popular theory that obstetrics is a more natural phenomenon among the less civilized people. I do not agree with him there. I do not believe that obstetric practice is any more successful or as successful among those ignorant people as it is among our more highly civilized races. I have been told by those who have lived among the Indians that the mortality from childbirth among the women is simply enormous. We hear only of the successful cases, and the fatal ones never come to our notice. The author has mentioned the curette I agree with him there, because I do not believe, particularly in a case of inflammation of the uterus that comes on two or three days after labor, it is possible for us to explore the uterine cavity with the finger. In a case of mine two or three weeks ago, in which I considered that the labor came on perfectly normally, I could not account for the infection. The placenta came away by the method of Credé, without the finger being inserted even in the vagina, and it seemed entirely sound. Two or three days afterwards, chill, fever, and some elevation of temperature supervened; the uterus was enlarged, extending up to the umbilicus. In that case it would have been impossible to explore the uterus with the finger. I used uterine douches, and the trouble abated. I believe the safest method, wherever the inflammation has extended to any great degree, is to resort to the curette. It need not be used violently, and a sharp instrument is not necessary. The dull curette, in early cases of puerperal sepsis, is, to my mind, the proper method. There is one other point that I would mention and that is the use of ante-partem douches I think the author of the paper is entirely right on that proposition. Unless there is some good reason for their use, I believe it better to let them alone. We cannot thoroughly disinfect the canal unless we resort to the methods he mentions, and the hot water has the effect of constricting the capillaries. It not only carries away the normal secretion, and does not always accomplish the object which you wish, but it produces a dryness of the canal, so that labor is usually rendered much slower and more difficult. For this reason I think it is much safer to let nature's processes alone in that particular, and to allow these secretions to remain there as a protection and as a lubricant.

Dr. J. F. McCone, of San Francisco: I feel that we have all gained information from this paper that is very useful. Unfortunately the doctor's suggestions are a little too elaborate for confinement cases amongst the poorer people. I have been doing considerable clinic work, and we are often called to attend a case where there is but one clean towel in the house, and no sheets at all. The preparation in such a case cannot be too elaborate. During some work that I carried on at the Rotunda Hospital, in Dublin, we followed a few very simple principles in the preparation of the patient, and in the preparing of the hands before taking the confinement case. The preparation of the hands consisted in a thorough scrubbing with soap and water, paying particular attention to the nails, and then afterwards a thorough rinsing, and immersing the hands in a bichloride solution 1:1000, and allowing them to remain there one minute by the clock. After the hands had been prepared it was necessary to be very careful to touch nothing but the patient. If you caught hold of the blankets, or attempted to move a chair or anything of that kind, the sterilizing process of the hands was to be repeated. No *ante-partum* douches were given Recent observations of Dr. Krönig have quite overthrown the idea that an antiseptic douche before labor is good. He showed that where an antiseptic douche was given, and a culture of streptococci and staphylococci were introduced into the vagina, the germs were found in abundance after 24 or even 30 hours. Where no antiseptic douche was used, and the germs were introduced into the vagina, the normal secretions of the vagina and of the cervix destroyed all the germs in less than eight hours. These observations were conducted over several months, and included 200 or 300 cases. Of course, where we have a history of a gonorrhea, or of a purulent or greenish discharge, I think it is a good idea to wash out the vagina, because in such cases we do not have the normal secretion of the vagina or cervix. We have a pathological secretion. The after-treatment in Dublin consisted in giving a vaginal douche of creolin when the temperature reached 100°, or giving an intrauterine douche when the temperature reached 101°; and as soon as the temperature showed a tendency to go up, to feed the patient milk-punches, everything that you could force into the patient in the way of food and sustenance, and to avoid religiously any depressents, like antipyrin, etc. The odor of the lochia also indicated at first a vaginal and afterwards an intrauterine injection of creolin. In the Rotunda Hospital it has often been observed that in cases that were confined in the street and brought in in an ambulance after the baby was born, there was, as a rule, no morbidity, and no increase in temperature after the accouchement. For that reason it seems plain that a great deal of the morbidity following confinement is due to infection introduced by the physician. The use of the curette after confinement and after abortion, particularly in cases of fever, seems to me to be very much abused. The great thing is to have the curette cut in the right direction. If you have a curette cutting towards the wall of the uterus in trying to clean out the uterus, you do not clean it out, you simply scratch it; you dig furrows in it; but if, on the other hand, you have the cutting edge of the curette directed towards you, instead of having a ploughshare you have a rake, and consequently can clean out the uterus. I do not believe in cutting into the wall of the uterus, but I think the curette intelligently used is a very good instrument in cleaning out the uterus. Instead of opening channels for infection by the use of the curette, if you use the proper kind, and use it properly, you drag out the abnormal contents of the womb and you allow involution to continue, and involution continuing closes instead of opens the sinuses and the lymphatics. It is the custom in the County Hospital when a woman who has had an abortion, criminal or otherwise, comes in, if she has a purulent discharge, or bloody discharge, to clean out the uterus. The cleaning sometimes can be done with placental forceps, but it is usually done with a curette.

Dr. C. E. Cooper, of San Francisco: In line with the paper, and also in line with the remarks of my friend Dr. McCone, I would like to call attention to a very fine paper by Leopold, of Dresden. He is in charge of a very large obstetrical clinic, and he tabulated his results for something like two years, and divided all the cases into three classes. First, he separates the normal from the abnormal cases as far as vaginal secretion is concerned; and these statistics refer only to the normal cases. He says at the outset with the normal cases he always cleanses the vagina for at least two weeks with some antiseptic. But among the abnormal cases those which made the best recoveries were those which had neither a *post-partum* nor an *ante-partum* douche. The second class, the next most favorable cases, were those which had no *ante-partum* douche, but were subjected to a *post-partum* douche. The statistics comprise something like 7,000 cases. The third class, those least favorable and making the least favorable recoveries, were those which were treated in the old way of both *post-partum* and *ante-partum* douches. I agree with the speaker who believes that infection is frequently carried into the vagina, and perhaps the uterus, by the use of the douche; not only from a douche-pipe that is unclean, but even through the water itself. In ordinary practice we are frequently called upon to give a douche, and it may be absolutely necessary, but if one notices the odor of the water coming from an ordinary kitchen boiler and compares it with water direct from the main pipes, he will find that the moderate heat to which the water in the boiler has been subjected has increased the foulness of the water to a very remarkable extent. This is particularly so in the fall of the year in San Francisco. The heat seems to cause the germs to multiply rapidly. If the only water available in the house is water that has been subjected to a very short period of boiling, or, as is more frequently the case, water which has simply been heated in the kitchen boiler connected with the kitchen range, the introduction of a few tablets into such water does not make it antiseptic, does not even make it aseptic, because it has been shown by Sternberg that a solution of 1:1000 of sublimate requires 15 minutes to destroy the germs which float in the air. A much safer plan, in my opinion, would be, if we are obliged to use such water in a douche, to take it as hot as we can get it, and let it stand at least 20 minutes, for the sublimate in it to attack the germs which are in the water itself before using it in a douche. Leopold called particular attention to the scrubbing of the outside parts, and the majority of his cases were carried through with entirely external examinations; and by practice I believe one can learn almost as much from the position of the child as he can from examining *per vaginam*.

Dr. E. E. Kelly, of San Francisco: I wish to make only one observation in regard to the after-treatment of these cases. I believe we are all subject to infection from germs, or exposed to germs, every day of our life, and the reason why we do not receive these germs into our system as pathogenic elements is simply because nature has provided us with a certain amount of resistance. I believe nature has provided for the vagina an antiseptic that is better than any antiseptic that we can use in the way of douches, especially *ante-partum* douches. As has already been said, there are other objections besides the fact that the natural antiseptic which nature has provided is removed. We also undoubtedly interfere with delivery by removing the natural lubricant of the vagina which assists in the passage of the child's head through the canal. As to the antiseptic in the preparation of the patient, the paper, of course, would be applicable, in cases where people have sufficient means to carry it out thoroughly. In the majority of cases in ordinary practice, I think that amount of elaborate preparation would be untenable and could not be carried out. I believe in thorough cleansing of the hands, but I do not believe that puerperal fever is due to the ordinary germs of infection. I think they are similar to the germs which produced the old-style "blood poisoning," but I do not believe that the germs which find their habitat in the vagina will produce puerperal fever. I remember an instance where a physician had five cases of puerperal fever, one after the other, all of which died. There is no doubt in my mind that the physician himself was the infective element in each one of these cases. If the germs which caused these severe and fatal cases of puerperal fever were the same which caused the simple rise in temperature after normal labor, it seems to me that they would find access to the vagina and produce fatal results very much more frequently than they do under the present order of things, because there is a traumatism in the canal, not only on the external genitals, but on the internal; at the site of the placenta there is a lesion for the reception of germs, and if the germs were present in the vagina, certainly infection would be exceedingly common. I think it is undoubtedly true that the severe cases of puerperal fever are caused by a germ of very much greater toxic effect than the ordinary germs found in the vagina.

Dr. J. H. Barbat, of San Francisco: As soon as we discover the temperature to be rising after confinement, the first thing to do is to make a culture of the lochia, and also a microscopic examination of the blood. We find, not in many cases, but in some cases, that the rise of temperature is not due to the infection in the vagina, but to malaria. It is proper in all cases to examine the blood. If we find the plasmodium of malaria, we want to administer the proper remedies. By making a culture of the secretions of the vagina we have a proper idea of what is causing the rise of temperature. If we find the germ of erysipelas, we know that we have a very severe case to deal with. If we find streptococci, we have also a severe case. Staphylococci produce probably the mildest form of infection in this case.

Dr. G. Gross, of San Francisco: There may be something in cleansing our own hands, and also those of the parturient. My own practice used to be to employ the method of Credé, but later on I found that when I left the expression of the placenta to nature I did better. Another point that seems to me of great importance is the discovery of the first signs of infection. The rise of temperature has been mentioned. I have found that there is one sign which is even more important, because it shows itself earlier. That is an imperfect involution. You may have no rise of temperature and still have an imperfect involution, which immediately is liable to call your attention to a process for which it is prepared, and the sooner you interfere the more chance have you to get ahead of the infection. Among the different remedies used for douching the uterus, Tarnier has drawn attention to a solution of iodine gm. 40, iodide of potassium gm. 6, distilled water gm. 1000. He claims that it is superior to bichloride. Another point, of which no mention has been made, is the treatment by permanent irrigation. I have had no personal experience with it, but I know that in some clinics it is used, and instead of using plain gauze, to use gauze permeated with tincture of glycerine.

Dr. J. C. Stinson, of San Francisco: In a case of mine the patient had been flowing for about ten weeks, and when I saw her the temperature had risen to about 101°; she was very anemic; the pregnancy was about between 5 and 6 months. After consultation we considered it wise to empty the uterus. We emptied the uterus, but the temperature kept up and ran up as high as 104°. The uterus was irrigated, and later on was curetted. The temperature kept up for about three days, going as high as 104°, and then gradually came down to normal, about five weeks after the operation. After the temperature had been about normal for two days the patient began to complain of pain in the left cornu of the uterus; she had a chill, and the temperature went up again. I saw her with the other physicians, the temperature had reached 102°, and on the third day it continued at 102°, but I did not think it was wise to interfere except to use the douches. The third day it went up to 103.5°. We examined the patient very carefully, and the inflammation seemed to be limited to the uterus and to the left tube. I advised that we perform an operation, which was agreed to The next morning the temperature was 100.5°. I removed the uterus and the tubes and ovaries by hysterectomy, ligating only the vessels. The patient died about 18 hours afterwards, and the temperature was 103.5°. If I had operated about four days earlier, before the temperature had become so high, I think, probably, I could have saved the patient. The operation revealed metritis, an abscess in the wall of the uterus, and in the left tube at the junction of the uterus. The autopsy showed peritonitis.

DR. VAN SLYCK, of Pasadena: I am very glad to have the paper receive such general discussion, because there are no points connected with the subject which do not need to be emphasized to thoroughly impress upon everybody's mind, the importance of thorough asepsis. There are no points that I wish particularly to refer to, except that I think it a very important thing that the patient herself should be thoroughly disinfected up to the vagina. It is my impression that that is the source of infection, many times the carrying germs from the patient herself into the genital tract. With regard to removing the contents of the uterus with the finger, there is that you can always ascertain with the finger whether the uterus contains clots and debris, and then by pressing down the fundus and by the use of a large, dull curette, if there are clots and pieces of membrane, or anything of that sort, they can be carefully removed with the curette directed by the finger. But if the debris is the consequence of infection, it is a kind of necrosed tissue that can be wiped away with soft gauze much more safely, I think, than by the use of the curette, because it is a constant exfoliation of necrosed tissue. It has been emphasized by some speakers that the curette really removes the barriers which nature is already setting up, and I think that point is certainly well taken. I cannot see that the curette is of any use early in the disease, and later, if it is a really septic case, the walls of the uterus are so friable that it is very unsafe to use a curette. I think the cases that get well after the use of the curette, get well in spite of the curette, and not because of it. Some of the best and most skilful operators have punctured the uterus after sepsis, and it is their testimony that they were not aware of the curette meeting with any resistance at all, the walls of the uterus were so soft and friable. It seems to me that it is a very dangerous instrument to use. A man should have the utmost confidence in his own skill, and have had a great deal of experience, to dare to use a curette in such cases after labor.

Modes of Examination and Operations Comparatively Safe and Easy per Vaginam.—A. M. TAYLOR, of Oakland, read a paper upon this subject. He said: Until recently, in order to examine obscure troubles of the pelvic viscera and to make a positive diagnosis, we had but one route, that through the abdomen by an "exploratory incision." There no longer exists an indication for celiotomy for exploratory purposes, or for the treatment of many diseased pelvic conditions. Most of these cases can be operated upon successfully through the vagina without the risk necessitated by the other route, the only objection being the limited space in which to operate. This advance in gynecological surgery, which we owe mainly to Pryor, of New York, Vanderveer, of Albany, and Richelot and Pean, of Paris, can be performed with much more rapidity and with very little danger of wounding vessels of any size. It is no longer the blind, bloody operation of years ago, but has been greatly improved in its *technique*. This has been well described by Pryor, in the *Medical Record*. After proper disinfection previous to the day of operation, and at the time, with the patient in the lithotomy position, the uterus is curetted and irrigated. It is then pulled down to a point at which the vagina is reflected from the cervix, as demonstrated by moving the cervix up and down, the point of reflection showing as a crescentic fold, appearing just at the point of the cervix as the latter is pushed up. Picking up this fold the operator cuts through the vaginal mucous membrane with scissors, the incision extending laterally on either side, being about an inch long. The posterior margin of this incision is grasped by forceps, and while traction is made upon this and upon the uterus, the operator passes his finger into the *cul-de-sac* to the level of the internal os. If the finger has not already perforated the peritoneum, the cavity is then wiped dry and the peritoneum is picked up with forceps and divided by scissors. A digital examination of the pelvic contents is now made. If an ocular inspection be deemed necessary, the operator introduces two fingers into the opening in the *cul-de-sac* and separating them laterally, tears the vaginal mucous membrane and peritoneum out to the point desired. The traction forceps being removed, the uterus is forced up behind the symphysis, and a gauze pad, to which a stout string is attached, is then passed into this opening and the patient is placed in the Trendelenberg position. By gentle manipulation the intestines are now made to enter the abdominal cavity. If adherent, they are gently freed. When the intestines have passed into the abdominal cavity, a complete diaphragm of gauze pads can be placed between the pus of the foci and the intestines above. In these cases in which the purulent accumulation is extensive enough to cause any degree of adhesions, an exploratory vaginal incision is generally sufficient to indicate the proper form of radical operation. By this means of exploration we can apply conservative measures to a class of cases which would be subject to great risk with any operation attempted from above. Absolutely and without mutilation we can make a positive diagnosis of the pelvic contents. The amount of room which is secured is amply sufficient for purposes of inspection. The *cul-de-sac* is entered in from one to five minutes. The blunt tearing of the vagina is made parallel with the branches of the vaginal arteries, and they are never wounded. The speaker, in conclusion, reported a number of cases illustrating, first, the advantages that would have been derived had they been operated upon *per vaginam*, and next, similar cases operated upon in this manner which had been more successful.

Some Improvements in Technique in Operative Gynecology.—C. E. COOPER, of San Francisco, read a paper upon this subject. He desired to report upon some of the newer methods which he had had the opportunity of observing in hospital practice, as he believed that his observations might prove helpful to others engaged in the same line of work. Regarding the anesthetic, after many cases of bronchial irritation and some of desperate collapse under chloroform, the use of the ordinary brands have been abandoned and that made from chloral adopted, with most satisfactory results. During the past four years he had seen two deaths from the use of ether in cases of undoubted kidney lesion. For that reason a chemical analysis of the urine should invariably precede its use. The A. C. E. mixture had not proved satisfactory. Nitrous oxide followed by chlorofom had proved most satisfactory. The inhalation of aromatic vinegar from a cone after chloroform or ether narcosis is of especial value in controlling the nausea of ether, and assists in bringing the patient to consciousness quickly. In some cases, particularly after chloroform, it fails to act so satisfactorily. One of the chief sources of pain to patients after abdominal operations is the movement of gas in the intestines. Distention of the abdomen and obstruction of the field of operation by the inflated coils of intestines may be almost entirely prevented by the following simple and effective plan: For three days prior to the operation the diet is restricted, sugars and starches being eliminated; $\frac{1}{60}$ of a grain of strychnine is given three times a day for at least a week before the operation. Of the laxatives, small doses of calomel, frequently repeated, and followed by sulphate of magnesia, is probably the most satisfactory. An enema was always given on the day of the operation. To further diminish flatulence, bismuth and charcoal with salol administered for one week before the operation was very useful. The Trendelenberg position, while one of the most valuable additions to gynecological *technique*, should not, however, be used in all cases, particularly in those where quantities of pus or blood are liberated in the abdomen. Before removing the patient from the table an enema of a half pint of hot normal salt solution is given. Whisky, one ounce, or black coffee, four ounces may be added if required, and repeated every six hours until peristaltic action has been reëstablished. This is a very valuable measure. It allays or prevents shock to a remarkable degree;

it prevents the tormenting thirst after laparotomy, and assists in restoring the normal blood volume. The concensus of opinion seems to be that codein is less objectionable than morphine. Trional to allay sleep is a very valuable agent. It may also be given by enema. After many experiments he had been much pleased with its good effects. In his observation cracked ice was not nearly as effective in allaying thirst as hot water. The preparation of the skin of patients was a detail too often neglected in gynecological operations. Suture material was often blamed for stitch-hole abscesses, which were really due to imperfect disinfection. Aqueous solutions of sublimate fail to penetrate the glands and follicles, but a 1:1000 solution in sterilized olive oil was perfectly effective, and he had yet to see the first stitch-hole abscess where it had been used. Regarding ligature and suture material, much improvement had been made of recent years. Catgut, no doubt, was an ideal material, but the difficulties in the way of its successful preparation were very great. Personally, he believed in a mildly antiseptic rather than in an aseptic catgut, for several reasons. Amongst these were the very great difficulty, if not impossibility, of rendering the catgut aseptic. Next, its presence, even when aseptic, in the wound, as dead animal tissue, afforded an excellent focus for the development of germs. The speaker expressed his preference for Edebold's formula in the preparation of catgut, which was simple and effective.

Abdominal Nephrectomy.—LUKE ROBINSON, of San Francisco, read a paper upon this subject, in which he briefly discussed the advantages and disadvantages of abdominal nephrectomy, as compared with the lumbar operation. Amongst the disadvantages of lumbar nephrectomy may be mentioned the small space in which it was necessary to work, especially in corpulent subjects. The difficulty of securing the pedicle and of ascertaining the condition of the ureter, as well as the dangerous traction upon the kidney to effect its removal. The fact that this operation is extra-peritoneal does not, in the present state of surgery, constitute an argument in its favor, nor can better drainage be obtained from the lumbar incision than from the ventral wound. The principal advantage of the abdominal operation is the possibility of obtaining information as to the condition of the opposite kidney. The facility with which the kidney is reached and the pedicle secured without traction upon vessels or adhesions is a strong argument in favor of the ventral operation. The choice of incision in an abdominal operation will depend largely upon the size of the kidney. When the renal tumor is large the median incision has some advantages over Langenbuch's operation. A modification of the operation that is strongly advised by Smith and others is an incision through the rectus muscle, near the linea semilunaris. The speaker then reported a case in which he had removed a floating kidney by the abdominal incision through the outer third of the rectus muscle. In this case recovery was uninterrupted, the patient leaving the hospital about six weeks after the operation.

Committee on Clinical Medicine.—The report of this Committee [published at p. 236], was read by the Chairman, JOHN FIFE, of Red Bluff, the subject being "The Diagnosis and Treatment of Typhoid Fever."

DR. W. S. THORNE, of San Francisco: It seems to me that the diagnosis of typhoid fever ought not to present so many difficulties to the ordinary observer. I think the German writers upon that subject have sufficiently set forth the fact that the thermometer is a very accurate method of diagnosing the disease. We have in typhoid fever, the prodromic period, the period of malaise, of headache, of pain in the muscles, of pain in the back, and of insomnia. Generally at some time during this period of invasion the patient is brought to the attention of the medical man, and he naturally observes the thermometric range. I think that if the temperature steadily rises from day to day, from morning until evening, and reaches 103° or 104° by the fourth day, we may safely assume that we have a case of typhoid fever to deal with; especially so when this thermometric range is associated with those symptoms of pain and headache and peculiar white light furred tongue, and frequently the diarrhea with peasoup stools. The condition in which we most frequently make a mistake is undoubtedly in so-called "typho-malarial fever," which is a misnomer. Malarial fevers are distinctly intermittent. Typhoid fever is remittent; and in the incipient stage, if the fever is intermittent, we may be quite sure that we have not a case of typhoid fever to deal with. The next fever with which it may be associated is septic fever, or the pathogenic fever of Murchison, sewer fever as it is sometimes called. It is an interesting fact that it was not until the first thirty years of this century that typhoid fever was differentiated from other fevers by actual *post-mortem* experience, and the pathological condition clearly and specifically set forth. This was done chiefly through the labors of Bretano, Chevalier, and Trousseau. Pathological researches have shown that typho-malarial fever without any mesenteric symptoms, is, as a matter of fact, a typhoid fever with the essential elements of mesenteric and intestinal ulceration. I remember, some years ago, I was a victim of typho malarial fever, at an elevation of some seven or eight thousand feet, in a frontier town, and it was called malarial fever by the doctors, and typho-malarial fever. There were no enteric symptoms at all. The belly is frequently even tympanitic in typhoid fever It my own case it was simply distended, and the bowels rigidly constipated. In that little mining camp there were 26 seizures of this fever, and 24 burials; 24 out of the 26 died. I was yet a minor; I had not yet reached my majority and was not yet a medical man, and from my recollection of those fevers, as I observed them on my own person and in others, I have no doubt that it was a typhoid fever with a malarial element possibly permeating, but having in fact an intestinal lesion. There is no disease with which I am acquainted that has so many clinical divergences as typhoid fever. I know of none so irregular, so erratic. You may have a fever lasting for ten days, aborting at the end of ten days suddenly, or you may have a fever which lasts from six to eight weeks. Kelly, of the Middlesex Hospital, London, who is a very excellent observer, and who has written a good many articles on typhoid, says that in these prolonged cases of typhoid fever you have what he believes to be a double infection; first, the infection of typhoid poison, and second, septic infection from the ulceration and sloughing of the intestinal and mesenteric glands. If that is not susceptible of demonstratien, it is at least very plausible and appeals to our reason and common sense. With the Woodbridge treatment I have had not the slightest experience. I think those of us who have studied the pathological history of typhoid fever, and examined, after death, the enormous ulceration that takes place in the small intestine about the cecum and at the ileo cecal valve, are convinced that the mere internal administration of calomel in any shape, either in infinitessimal doses or large doses, can have but a very inadequate effect to control this ulceration, which is the essential characteristic of this fever. I have used calomel according to the advice of Wunderlich and Widermeister, and I have been convinced that in the early stages, that before the ninth day, the administration has resulted to the benefit of my patient, but that the mere administration of calomel in typhoid fever, the essential lesion being ulceration with all its attendant dangers, as a curative agent, is contrary to our reason and experience. I do not believe that it cures any cases of typhoid fever. Regarding the treatment of Brandt: A good many years ago I went to Germany, and I came home very much impressed with what I had seen and heard respecting it. I lived in a community where there were the average number of typhoid cases, and I began, as near as I could—and it was frequently very difficult in private houses and private practice—to carry out the directions of Brandt, in his water treatment of typhoid fever. I had, in a very short space of time, three deaths before the ending of the second week. I abandoned this practice in the treatment of typhoid fever and I have never resumed it from that day to this, and I cannot say that I regret it. In St. Mary's Hospital, where I see, during the course of the year, a great number of cases of typhoid fever, our method of procedure is, when the temperature passes 102° or 102.5°, to apply an ice cap to the head, the base of the brain, to the cranium, to the summit of the head. When we give an antipyretic, we have found that 5-grain doses of phenacetin combined with quinine is one of the most efficient that we possess. We have an exceedingly small mortality at St. Mary's Hospital. We have had, in the past year, two cases of violent hemorrhage in the course of typhoid fever, both of which came to us from the Sierra Nevada Mountains. Both recovered. The statistics of Brandt are certainly seductive. He tells us that in something like 212 cases of typhoid fever, he had no deaths, and he does not believe that if his system is carried out rigidly, there will be any deaths from typhoid fever. When people believe that theory, it is certainly their duty to themselves and to the community to practise it.

DR. E. E. KELLY, of San Francisco: I am a firm believer in the Woodbridge treatment, from my experience with typhoid. According to my understanding of it, it is not the calomel which is expected to produce the antiseptic effect, but the guaiacol carbonate. The capsule which is given after three or four days, contains 4 or 5 grains, and is given every four hours. About six weeks ago I was sent for in the night, the patient had a temperature of 104.5°, and the same afternoon, after placing him in the hospital, he had a temperature of 106°, which you will all admit is very serious. He had been ill for some time, a week or two, and was probably in the second week of the disease. I immediately put him on the Woodbridge treatment. He had at that time tympanites. He had a very bad tongue, sordes on his teeth, eruptions on his body, and all the characteristic symptoms of typhoid fever. I used this method strictly in accordance with Woodbridge's suggestions, not because I think that it is the perfect method, but because it is the method he has used with good results, and because I did not know any better method. In five days the temperature was reduced until it did not go on any day above 103°. In addition to the Woodbridge treatment I also use high enemas, a quart of water which had been boiled, with 15 drops of carbolic acid, asking the patient to retain it as long as possible, and the reduction of temperature which follows is remarkable. In an hour to an hour and a half after the use of this treatment the temperature would be reduced from one to two degrees. In no case except one have I had any symptoms of carbolic acid poisoning as shown by the discoloration of the urine. In one case where I used 20 drops, and used the enema twice a day, I had some little toxic effect. I have had a number of cases that I have treated, using the Woodbridge treatment as nearly as I possibly can. I use the tablets in the beginning every 15 or 20 minutes, until there are five or six copious stools during the day, then gradually reduce it, and in 48 hours beginning the use of guaiacol carbonate. The cold sponging and the ice cap which has been suggested to-day is a very grateful treatment to the patient, and one which has a very decided effect upon the temperature. Since I have begun this treatment I have had no deaths at all, and in four or five days after the beginning of the treatment there is always a moistening of the tongue, a disappearance of the sordes from the teeth, disappearance of the tympanites, and of all those disagreeable symptoms which have made typhoid fever such a painful and distressing as well as dangerous disease. The first case that I mentioned had also some hemorrhage from the bowels. I am also a believer in the method of diagnosing the disease by means of the bacillus, and I have had one case which causes me to be a more firm believer than I was before. I had consultation in this case, and by every means that we could devise we could not conclude otherwise than that the patient had typhoid fever. There was a gradual rise in the temperature, and a characteristic temperature curve; there were all the symptoms, though there were no petechiæ. By means of the culture we attempted to make the diagnosis, and we were not able to find the bacillus. For that reason we concluded that perhaps there was a mistake in the diagnosis, and as the patient had complained continually of pain in the back, and I was led to believe there was some trouble in the right side in the region of the ileo-cecal valve, we concluded that it would be best to make an incision through the back and see what we could find, and upon cutting down there I opened into an immense abscess, which immediately began discharging fecal matter. I have no doubt that this abscess was connected with the appendix in the beginning. The patient made a perfect recovery.

DR. E. H. WOOLSEY, of Oakland: I was very much pleased to hear the remarks of Dr. Kelly with reference to the use of the carbolized water enema, because I have found, from a long experience, that enemas are of much greater value in the treatment of typhoid fever than anything else. I learned, ten years ago, after *post-mortem* examinations in cases of death from hemorrhage, that those hemorrages occur at the head of the colon, in the cecum. It then occurred to me that there was a ready method of preventing that. So I began the wash-out treatment, using a medicated enema, an emulsion of turpentine, and putting in some salicylate of soda with a view of antisepsis. More recently I have come to the use of simple sterilized water, and I find that it answers just as well as the various medicated enemas which I formerly used. It can be used with greater facility, with less inconvenience to the patient, and, I may incidentally add, with less destruction to the syringes that are used therefor. I find that the temperature is not only controlled by the emptying out of the colon, but by the temperature of that water. I have come to use water cooled to below the temperature of the body; and if the temperature is very high, we are able to control that temperature almost perfectly by using water of tepid temperature. The rationale of this treatment seems to me to be perfect. The cause of the fever is undoubtedly septic, as Dr. Thorne has remarked, and probably also from ptomaines that are absorbed from the bacilli. If you get rid of this fermentation and get rid of the toxines, you will have no fever. The Woodbridge treatment is, in my opinion, a scientific treatment, because it serves to eliminate and at the same time antisepticize the contents of the bowels. I criticize that treatment because of the somewhat quackish presentation of it. I think that calomel exhibited in small doses, frequently repeated, would probably be as effective. I think that a thorough washing out of the colon will serve almost every purpose, and in my later cases of typhoid fever I have used very little other treatment; scarcely any, other than to quiet them if they are delirious. After you have once started giving them two or three enemas in 24 hours, of one, two, or three quarts, what they can stand, the temperature is most thoroughly controlled I can show you probably a hundred charts where the temperature curve has fallen immediately after the wash-out treatment, and could be kept under perfect control. If the temperature should rise two or three degrees in the day, by giving a wash-out treatment you will surely have a fall of from one to two degrees, and you can keep the temperature down to the proper degree, avoiding all nervous phenomena. With respect to the administration of this enema: the ordinary method of giving an enema does not wash out the entire colon, by any means. You may accomplish that, probably, by the use of the tube, but that is somewhat dangerous, perhaps, and sometimes a little difficult. You can do it perfectly by gravity. Raise your patient's hips or raise the bed so that the incline will be towards his head, and then let the water flow in through a fountain syringe until he is pretty well filled up, then turn him to his right side, the water will flow over to the head of the colon; then by lowering the bed, or by lowering the body, if it is a child or some one that can be handled easily, the fluid will pass down well into the cecum; then by raising the bed again, while the patient is still on his right side, you wash out and there flows back a mixture of whatever there may be in the colon. When you clean out all below the ileo-cecal valve, the matter above that point in the ileum will come down readily, and so you can get rid of the retention of the fermenting matter about Peyer's patches.

DR. E. B. ROBERTSON, of Jackson· One of the best things in the treatment of typhoid fever is to flush out the bowels abundantly with hot water or with soap and water. Elmer Lee, of Chicago, claims that he can introduce from one to three gallons at a time for that purpose. Typhoid fever is an enteric disease, and it is septic in character, Peyer's patches being involved. This flushing out as far as we can possibly reach with the use of a syringe, can always be accomplished by placing the patient on the right side, with the hips slightly elevated, and turning him to and fro. The fever is not the disease; it is only the product; and, if we want to mitigate the fever, the external application of tepid water is useful, but best of all is the internal and abundant use of water.

DR. R. BEVERLEY COLE, of San Francisco: It would seem to me that this paper is limited entirely to new-fangled notions. This voice of mine may be construed as being one from the grave; it may be construed as enunciating those methods and that system which have long since been buried; but, I trust that none of you are yet so bold but that you can see, even in the past, that which has value. We know this disease and its means of communication. We know its phenomena, and it would seem to me not so difficult a matter as has been represented in this paper to even diagnose and differentiate a case of typhoid fever from other forms of fever. Admitting that this fever is due to a germ, that germ has vitality that it is almost impossible to overcome and which I deny as being possible to overcome with carbolic acid in any strength that it is admissible to administer. Todd, who has lived before us and who had, perhaps, a larger field and instituted closer investigation of this disease than any man of whom we have any knowledge, even at the present day, recognized that the great principle involved in the treatment of typhoid fever was to sustain the system whilst the great mother, if you please, the *vis Medicatrix Naturæ*, had an opportunity of eliminating the poison and thus giving to the individual some hope of salvation. If we would recognize that indispensable principle in the treatment of all typhoid forms of disease, of supporting the system and giving nature an opportunity to eliminate the poison, our results would be quite equal, and in my judgment, from observation, superior to those which you young gentlemen to-day believe to be the only treatment that should be instituted in these cases. I have seen so many, although, as you know, I am not a specialist in fevers, yet my experience has been so large that I feel I am entitled to be heard, and I unhesitatingly assert, without fear of successful contradiction, that this great principle of giving nature an opportunity is better than all that has been suggested.

DR. GEO. L. COLE, of Los Angeles: I have never been guilty of using the Woodbridge treatment, simply because I believe it to be unscientific. As I understand it, Dr. Woodbridge himself does not know which one of the many elements in it is the cause of the success which he claims, and while the treatment of this fever is in a chaotic condition, there are a few things to which we can hold fast, and which we younger members, I think, should retain as our stronghold. At the time Woodbridge began his so-called treatment, while I did not adopt that, I did begin the use of intestinal antisepsis, and, without taking up your time, I am confident that I can say that I have carried my cases of typhoid fever through, shortened their duration, and gotten them through in better shape by the use of intestinal antiseptics. The one in particular that I have been using has been salol, from the fact that it is an antiseptic which will pass through the stomach and dissolve into its constituent parts in the intestine and there act as an antiseptic. There is one point about the Brandt treatment which has seemed to me to make it a rational treatment, and that is the fact that all scientific investigators are satisfied that where it is carried out faithfully, the toxicity of the urine is greatly increased, increased over that under the employment of any other method. We believe that the fever is due to a septic condition, and we find that this septic condition is carried off more fully and more quickly by the use of the Brandt method than by any other. I believe that a rational treatment of this disease is a combination of the Brandt method, and with that, if you choose, the irrigation of the alimentary tract through the rectum, which I believe is a very important element, combined with intestinal antisepsis.

DR. FIFE, of Red Bluff: I have lived for about fifteen years in a climate where malarial fevers prevail, and I wish to say that it is a mistake to suppose that malarial fevers are always of an intermittent type. They are frequently remittent and frequently continuous, and the temperature mentioned by Wunderlich is very rarely seen. I have seen it on one occasion. In the great majority of cases it does not occur, and the ease with which typhoid can be diagnosed during the first week is not as great as we have been led to believe from the discussion. In regard to the Woodbridge treatment, my opinions are based upon personal experience somewhat and upon the opinions of Delafield and others. I believe that it has no merit whatever. In six cases which have come either directly or indirectly within my observation, there have been 50 per cent. of deaths, and the treatment was carried out literally. I believe, as I said in my paper, that intestinal antisepsis would seem to one who is accustomed to surgical work and who is familiar with the difficulties attending rendering the hands aseptic, rather a hard matter to attain. The intestines are 25 feet in length, and to render them aseptic by means of any quantity of medicine is not easy. It does not seem to me that it can be done; maybe it can, but I do not believe it. I do not believe there is any treatment now known that will abort typhoid fever. That is the claim of Woodbridge. He says that in the majority of cases where the treatment is commenced early it can be aborted. He has given statistics of several hundred cases, and I believe at the time he was here at the American Medical Association meeting he had been using it for a period of 13 years without a death. I do not wish to dispute his statement, but it seems to me they are rather remarkable. If his cases were all typhoid fever, they must have been of a very mild type. As Delafield says, in the mild cases it may exercise some shortening influence on the course of the disease. In severe cases it exercises no influence whatever.

Syphilitic Diseases of the Liver, with a Report ot Cases.—GEO. L. COLE, of Los Angeles, read a paper upon this subject. He said his reason for bringing it to the attention of the Society was the belief that some cases of syphilitic disease of the liver are mistaken for ordinary cirrhosis or other obscure disease of that kind, and that lives might be saved by appropriate treatment that would otherwise succumb. This opinion was based upon two cases that had been treated for cirrhosis of the liver and Bright's disease, by several competent men, and they were only saved by large doses of iodide over a long period of time, aided by mercurial injections and inunctions. In one of these cases the speaker had accidentally discovered an early syphilitic history which had been withheld from the former attendants. This, with the true picture of cirrhosis presented, rendered pardonable the mistaken diagnosis. The majority of text-books on general practice either omit this subject entirely or dismiss it so briefly that attention is not sufficiently drawn to it. Loomis devotes two pages to it under the head of "Gummy Tumor of the Liver," Osler a page and a half in which, among other things, he says: "A second group of patients, also anemic, pass large quantities of pale urine containing albumin and tube casts." This was a picture of one of the speaker's cases in which the affection had been diagnosed as Bright's disease, and so treated for a time. Strumpell aptly remarks, "whether we feel certain of syphilitic hepatitis or simply suspect it, antisyphilitic treatment should be tried." The speaker then reported the two cases already mentioned, one of which closely resembled cirrhosis, and the other equally Bright's disease. Both had recovered under strictly specific treatment.

Committee on State Medicine and Hygiene and Adulteration of Foods and Drugs.—The report of this Commitee was read by the Chairman, THOMAS D. WOOD, of Palo Alto, his subject being "Popular Education and Sanitary Progress." The speaker said progress in matters relating to hygiene and public health is more rapid than ever before. Investigations in the field of preventive medicine are being pushed with vigor and success. Everything which affects the health and life of man is being subject to careful study. Much has been done to decrease the mortality due to tuberculosis, by isolation and by the prevention of contagion from diseased food. Much has been done to provide for the people purer milk, better and cheaper water, and unadulterated food. Permanent sanitary progress depends upon two factors: (1) The execution of health laws embodying important conclusions on sanitary science, and their enforcement by competent officials. (2) Popular education, thereby affording intelligent coöperation on the part of the people in all matters pertaining to the public weal. In this country, in the long run, everything depends upon the average and the ordinary citizen. The development of the individual must keep pace with and precede the growth of sanitary regulation and control. General intelligence alone will make possible the individual hygiene of the consumptive so important to the welfare of the patient and to those about him. A significant example of the lack of appreciation of sanitary measures is shown in the attitude of many people, even of average intelligence, towards vaccination. It is not strange, perhaps, that the almost uncivilized foreigners in our great cities should resort to riot and bloodshed to escape obedience to health ordinances, but that the members of some of the most intelligent communities should seriously oppose vaccination of all the school children, seems almost incredible. It therefore seems that there should be a steady campaign of education vigorously and steadily carried on for the enlightenment of the public. This work of education could be accomplished through many channels, several of which were enumerated by the speaker. Some one said, years ago, "Everybody ought to know as much about health as the doctors know, and the doctors ought to know a great deal more than they do." Too frequently the doctors know more about disease than about health. Physicians should be enthusiastic educators in the department of hygiene, not only through formal lectures, but in daily contact with their patients. Physicians should help to educate people to the point where it will be quite as much the duty of the medical profession to keep the people well as to cure them when they are sick. In conclusion, the speaker said hygiene seems to be the most neglected subject in the curriculum of schools, and less important than none. Popular education, wherever and whenever possible and practical, should be the watchword of sanitary progress.

Adulteration of Food Products.—F. H. PAYNE, of Berkeley, read a paper upon this subject. He said the fact that the proper authorities are looking into this subject and taking legal measures in relation thereto, does an enormous amount of good. It educates the people, and it actually stops a large amount of sophistication. For several years past Prof. Hilgard and his assistants at the State University have done a great deal of work in this line. It has been found that many of the articles sent in are not adulterated to any illegal extent, while others bear evidence only of faulty manufacture. Tomatoes were amongst the worst of the food products analyzed. Nearly all were found to show the presence of salicylic acid and similar preservatives which would be detrimental to healthy digestion. The Board of Health of San Francisco has done a great deal of excellent work in the matter of milk adulteration, which had been carried on to an alarming extent. Many articles of food have been examined by the Board's chemists. Amongst the worst of these was the so-called "concentrated cream." The evaporated cream, so-called, does not exhibit a trace of that article. It is simply milk evaporated from three parts to one part in weight. Condensed milk is also largely adulterated. In many cases glucose was added. This agent was also used in the production of commercial honey. Fruit jellies seem to offer a fertile field for adulteration. Of 33 samples of currant jelly examined only 9 were pure. One ingenious product consisted of apple jelly and starch paste, being colored with aniline dye and flavored with acetic acid. In conclusion, the speaker said, "it is one of the strange developments of modern commerce that, in a land like California where fruit is so abundant, such adulterations can be profitably sold. We let thousands of tons of fruit go to waste every year in our orchards and in our market places; and yet import from abroad pretended fruit products which are not only adulterated, but which are absolutely injurious to the health of the people."

Committee on Medical Education and Medical Legislation.—WM. ELLERY BRIGGS, of Sacramento, Chairman of the Committee, read its report. He said that little progress had been made in medical legislation during the last session of the Legislature. The causes which made it impossible to secure the attention that medical matters demand were the shortness of the session and the lack of medical men in the Legislature. Medical men were urged to take more interest in politics and in legislation. Our political condition needs the earnest and honest effort of every intelligent citizen to wrest the power from the hands of the bosses and spoilsmen. The aid of the medical profession is also greatly needed to carry out legislation in correcting sanitary abuses and in securing proper laws to regulate the practice of medicine. The people would thus be protected against the great numbers of ignorant men who are driven out of other countries and several of the eastern States, which examine every applicant for a license in regard to his qualifications to practise.

Committee on Medical Jurisprudence.—A. M. GARDNER, of Napa, Chairman of the Committee, presented this report, the title being "A Review of Article III of the New Lunacy Law." The speaker said it would be interesting to review the entire Act establishing a State Lunacy Commission that had been passed by the last Legislature and duly signed by the Governor, but as time would not permit he desired only to consider that portion which pertained to the commitment of the insane. The speaker alluded first to the conditions that had obtained in the past in connection with the care of the insane prior to their commitment and subsequent thereto, previous to entering the asylums. This he characterized as exceedingly bad, and quoted from an article that he had written for one of the daily papers commenting on these abuses. The speaker pointed out the remedies that had been provided under the present enactment. Special provisions for the examination of the insane had also been made. These could now be conducted out of Court, a humane and wise provision, which would, in many cases, directly assist the recovery of the patients. The

Act also provides that all the testimony in connection with the commitment of the insane patient shall be reduced to writing, and presented to the Superintendent of the asylum to which the patient has been sent. Copies of these are also filed with the State Lunacy Commission. This he believed was a great improvement. Under the old law the details have been exceedingly meager and unsatisfactory, as evidence of the carelessness of the Medical Examiners. The speaker then quoted at length from a former article [*vide* MEDICAL TIMES, vol. vi, p. 481], in illustration of the vagueness that often characterized the information that accompanied patients to the asylums. The speaker then referred to other safeguards that were guaranteed the friends of the insane, and the patient himself, both prior and subsequent to his commitment. In conclusion, he said, "perfection is rarely attained in any of the affairs of human life, but I most sincerely believe that the law meets most of the exigencies that arise in the commitment of the alleged insane, and that, at the present moment, no state or country on the face of the earth has a law in the exercise of which the rights of those unfortunates can be so humanely guarded and protected."

DR. E. H. WOOLSEY, of Oakland: I think we should congratulate ourselves as medical men and as citizens of this State, that this law referred to by Dr. Gardner has been passed. No one knows the need of such a commission better than Dr. Gardner, who is himself at the head of the largest institution for the insane in the State. Some year and a half ago I accidentally was present at a meeting of the Directors of the several insane asylums of this State at Sacramento, where Governor Budd was making an effort to render uniform the methods of administration in the matter of blanks and reports, and various economical matters, and I then suggested to a Director this very idea of establishing a Commission whose duty it should be to supervise the various asylums. I believe that was the first time the idea was suggested. I am very glad that it has developed fruit. It is a singular fact that those who are concerned in the care of the insane, come after a time—and perhaps I might say this of all eleemosynary institutions—to consider that the whole machinery of the government and of industrial affairs should be concerned and conserved to their ends and the care of their inmates. I submit that the increase of insanity in this State is so great that the time is surely coming, unless the methods of curing and caring for the insane be very greatly improved, when the sane people will be very much fewer than the insane, and when the sane people, to borrow a metaphor of Bierce, will have to build protective asylums for themselves to keep them from the insane, to say nothing about the financial question which will be involved prior to that time. Victoria Woodhull, some years ago, had the courage to advance the idea that we should stop the procreation of the insane, and to do that we should castrate the men committed and also perform oöphorectomies on the women who are under the menopause. I would submit seriously, for the good of this State, that question to this Commission. I take issue with Dr. Gardner, in his criticism of the reports sent in by the examiners. I do not think it is correct to say that because a man has no suicidal or homicidal tendencies, he is not dangerous to be at large. If a man walking down Market street should suddenly take it into his head to stand on his head, I think it may be reasonably assumed that that man, though he may not have exhibited any homicidal or suicidal tendency, is dangerous to be at large, because the next instant he may turn somebody else on his head.

Report of the Board of Examiners.—This report was read by the Secretary, C. C. WADSWORTH, of San Francisco. The number of meetings held during the year was 12; certificates granted, 282; applications refused, 4; applications withdrawn, 1. The report of the Treasurer of the Board showed that the total receipts for the fiscal year, there being no balance on hand from the previous year, had been $2,782.50, the total disbursements being $2,782.50. The receipts showed that the revenue from advertisements in Register and sale of copies of Register had been $1,322.50, and from applications for certificates, $1,460.00. [The financial portion of the report was referred to the Executive Committee.]

Report of Secretary.—WM. WATT KERR, of San Francisco, the Secretary of the Society, presented the following report: The roll as corrected to March 31, 1897, shows 312 active and permanent members in good standing. Since last meeting 2 members have died, 3 have resigned, and 27 have been dropped from the roll for non-payment of dues.

Report of Treasurer.—Thos. Ross, of Sacramento, the Treasurer of the Society, submitted his annual report. The report showed that he had received during the year $3,591.67, and disbursed $1,150.33, leaving a balance on hand of $2,441.34. [The report was referred to the Executive Committee.]

Selection of Place of Next Meeting.—Under this order of business Del Monte was suggested by Dr. F. B Carpenter, but was not supported, the usual objections being raised against. Dr. R. Beverley Cole proposed Fresno, and Dr. W. T. Maupin of that city spoke in its favor. There being no opposition Fresno was chosen.

Election of Officers.—The Society then went into the election of officers, with the following result:

President	C. L Bard.
First Vice-President	Wm Watt Kerr.
Second Vice-President	E. H. Woolsey.
Secretary	Wm. Fitch Cheney.
First Assistant Secretary	H. Bert Ellis.
Second Assistant Secretary	C. C. Wadsworth.
Treasurer	Thos. Ross.
Board of Censors	W. P. Maupin, Chester Rowell, W. Le Moyne Wills, G. A. Hare, C. A. Rodgers.
Board of Examiners	C. C. Wadsworth, J. H. Barbat, W. Dodge, E. E Kelly, A. P. Woodward, F. B. Carpenter, W. E. Hopkins.
Alternates	J. M. Williamson, D. A. Hodgehead, H. H. Hart.

Prize Essay.—In the absence of the Chairman of this Committee, Dudley Tait, of San Francisco, the report was presented by B. F. Clark, of San Francisco. He said but one paper had been presented for the consideration of the committee. The title of this paper was "X Rays in Medicine," and its author Dr. P. M. Jones, of San Francisco. The committee desired to specially commend the paper for its brevity, and for the great amount of original and scientific research to which it bears evidence. It was recommended that the Society's prize be awarded to the author. [The Society concurred in the recommendation.]

Committee on Publication.—A. P. Woodward, of San Francisco, Chairman of the Committee, read this report. He said: Your Committee on Publication begs leave to say that the Transactions of the Twenty-sixth Annual Session were issued after the usual delays. It seems remarkable how many causes for delay appear, as month after month passes. After special written requests, even the papers of some chairmen of committees fail to be furnished. The discussions were practically published in full. The speaker summarized the mode of distribution of the Transactions and the expense in connection with same, the total of which amounted to $592.80.

Evening Session.

A Practical Demonstration of the Vidal-Pfeiffer Typhoid Serum Reaction.—J C. Spencer, of San Francisco, gave a practical demonstration of Vidal's test for typhoid fever. He said: In 1895, Vidal, of Paris, continuing on the line of experiments conducted by Gruber, Pfeiffer, and Kolle, believed he had discovered a reliable means of diagnosing typhoid fever in its early stages. The value of the test depends upon the presence in the blood, and in certain secretions, of an albuminoid body called agglutinin or paralysin, that, when brought in contact with a culture of the typhoid bacilli will, according to the degree of concentration, and modified by the virulence of the particular colony, be followed by certain phenomena. Active movements of the bacilli would first be slowed, certain individuals will cease to move and others will group themselves into relatively large or small masses. It is this grouping or "clumping" which constitutes the diagnostic value of the test. The earliest date upon which it has, so far, been recorded that this reaction can be obtained, is the third day. The length of time that this agglutinative property remains in the blood has been variously observed as seven years, seventeen and a half years, and thirty-one years. At first it was considered necessary to procure a few cubic centimeters of the blood of the patient to obtain the serum, but it has since been ascertained that a drop of dry blood rubbed up with distilled water will give the reaction. The nature of these agglutinative bodies is essentially albuminoid; they have been assigned by Vidal and others to the globulins. Experiments on several hundred cases have established the reliability and certainty of this test. Its value as a diagnostic aid is immeasurable. In conclusion, the speaker said the inference most natural to be drawn from the action of this agglutinative substance, outside of the body, is the application of the same, as such, or in a somewhat modified form, as a prophylactic or as an abortive after typhoidal infection has once been established. If applicable in a case of typhoid, it would seem as though some simple method would be evolved for the prevention and cure of all the acute and infectious diseases. Unless there should be a sudden and radical reversal of the hitherto accepted views and methods of procedure, the evolution will, in all probability, take place along these lines.

The Cure of Tuberculosis with Oxytuberculin.—J. O. HIRSCHFELDER, of San Francisco, the author of this method of treatment, gave an extended review of it, which was illustrated by demonstrations on cultures, animals, and patients. He referred first to the method of treatment of tuberculosis by oxytuberculine, as described in the OCCIDENTAL MEDICAL TIMES, of November, 1896. Since this publication he had lengthened the period of operation of the peroxide of hydrogen upon the tuberculine, the sterilization being now continued for 120 hours. Since using the oxytuberculine so prepared, better results have been obtained than with the lymph as formerly made. Tables of cases treated were shown, many of the patients having been seen by other physicians before and during the treatment. All cases, even those who were in a dying condition when treatment was begun, were presented, and the speaker stated that he had striven to make the showing rather less favorable than more so. No case was considered cured unless the bacilli of tuberculosis had entirely disappeared from the sputum and the condition of the lung was found to be normal by physical examination. Sixteen cases in the last stage, treated by the old method, showed 1 much improved, 2 slightly improved, 2 unchanged, 6 worse, and 4 dead. Eleven cases in the last stage, treated by the new method, showed 1 cured, 5 much improved, 1 slightly improved, 2 unchanged, 1 worse, and 1 dead. In the third stage, of 25 cases, 2 were cured, 18 much improved, 2 slightly, and 3 unchanged. Of 9 in the second stage, 4 cured and 5 much improved, and all of 4 early cases were recorded as cured. Culture experiments made with mixtures of oxytuberculine with veal bouillon proved that this action was due to the prevention of the growth of the germ by the presence of the remedy, although other germs flourished in the liquid, proving that the action was a specific one upon the bacillus of tuberculosis. He said: There is no other substance known which acts in this manner. It is possible by this inhibitory action upon the germ, to determine the strength of each preparation of oxytuberculine. Photographs of cultures showing the gradually increasing inhibitory influence of increasing strengths of oxytuberculine were shown. A similar inhibitory influence of the oxytoxines of diphtheria, typhoid staphylococcus pyogenes, and micrococcus upon the respective germs was proven, and cultures demonstrating this fact were shown.

DR. DAVID STARR JORDAN, President Stanford University: About 25 years ago, when the discovery of the huge forest of invisible plants that surrounds us, inside and out, was first made clear, Prof. Tyndall called attention to the fact that the ravages of the bacteria of microörganisms were far greater than the ravages of war; that everywhere, wherever there were men, these creatures were at work, and on every battlefield they were removing the dead bodies faster than the elements—and the elements themselves are simply the operations of these bacteria—could do. Prof. Tyndall called attention to the fact that now that we know what our enemies are, we will learn their nature, and then we can fight them. This splendid work of Dr. Hirschfelder marks one great step, one of the great many that have been made by different men, but the last one, and one of the greatest of them, I believe, one of the greatest steps in the direction of fighting these animals and plants and subduing them and getting rid of them. In other words, we have learned what our enemies are, and now, having learned what they are, we know how in some degree to protect ourselves from them.

DR. JOSEPH LECONTE, Professor of Biology University of California: I had thought that President Jordan, as opener, would have given you something of an address on the subject. So far as I am concerned I had expected to play a second fiddle, and in fact a comparatively small one. I have comparatively little to say, because I have only comparatively recently looked into the subject at all. We have all been intensely interested in what we heard of what Prof. Hirschfelder was doing. I was so much interested that I came over and examined his experiments. Let me say in the first place that there are three questions involved. One is the chemical question; one is the biological question, and the third is the clinical question. In regard to the first, all of these are intensely important, but the greatness of their importance is in the order in which we have named them. On the chemical question I have nothing to say. I will only make this remark: that this is apparently so new a departure—if it is a real one—that nothing that we have had before in the way of antitoxins by analogy throws any light upon it. With that one single remark I dismiss it. In regard to the last, namely, the therapeutic or clinical question, on that also I shall have nothing to say because you are the judges of this. Some of you perhaps have followed these clinical experiments, but at any rate you are better able to judge of them than I am. The only one that is left, therefore, is the biological question. All that I can say is simply this: that I have gone over these experiments, not performed them myself, but as detailed to you by Prof. Hirschfelder. I have, however, plied him with questions. I have examined carefully all of his preparations. I have put objection after objection in order to draw out his answers. I was skeptical at first. I cannot say that I am entirely satisfied yet; but I was as much convinced as I could be by simply the telling of one person to another, and examining carefully the results and putting questions and objections, as much convinced as I could be with any set of experiments which I did not personally engage in and follow step by step myself. That, of course, is all that I can say. To all appearances it seems to me that the oxytuberculine does inhibit the growth of the tubercle bacillus in culture fluids. Now, the inhibition of tubercle bacilli in culture fluids is one thing; the inhibition of the tubercle bacillus in the living body is, we all admit, another thing. The conditions are different. But undoubtedly the inhibition of tubercle bacilli growth in culture fluids gives a strong hope that it will also do so in the living body. Whether it does or not you are to judge, and not me. There is but one, and this seems to me the real gap, wanting in these experiments, and that could not be helped. It is the performing of experiments where we can control the conditions, where we can have sure control of the cases and experiments upon them at the same time without the use of the oxytuberculine in animals subject to tuberculosis. That is really what we ought to have. That would be conditions exceedingly similar to what takes place in the body. If we inoculated animals subject to tuberculosis, or had them naturally infected with tuberculosis, and had other animals similarly treated, some with the oxytuberculine, and others without the oxytuberculine, and watched the effects parallel to one another, we cannot do that with a human patient. It is that that makes experiments upon animals so exceedingly important in cases of this kind. This, of course, could not be done. A man in practice without an institute for the purpose, how is it possible for him to carry out these experiments properly? And this brings to my mind now, what a pity that we had not a Pasteur Institute in San Francisco, and a man like Dr. Hirschfelder, who has shown the true spirit of science, to carry on investigations there. Will not this, by its enormous, overwhelming importance, be a spark to the State, to the city, and to wealthy enterprising individuals to come forward and establish something of that sort in our midst? Surely, if there is anything, as it seems probable that there is, in these experiments, the glory of them would reflect not only upon Dr. Hirschfelder himself, but upon the city of San Francisco, the medical profession of California, and perhaps a little modicum, infinitessimal, perhaps, upon myself as the teacher in zoology of Dr. Hirschfelder.

PROF. WM. E. RITTER, of the University of California: In the few minutes that I shall take—the hour is getting late—I want to make three points. The first one is that Dr. Hirschfelder ought, in some way, at the expense of the public and State, to have at his command five years, at least, of his own time, with as many chemists and as many bacteriologists as he needs, to carry out these experiments in whatever way he should see fit. I wish to add my voice in emphasis of what Prof. LeConte has said in this matter. Another point that I wish to make is one that is very obvious to every biologist and every scientific physician, and becoming more and more so every day, that every one of these problems, in which the medical profession is most intensely interested, is not only a medical problem, but it is a biological problem. The other point that I want to make is in the way of a suggestion from a biological point of view entirely, of the rationale of the operation of the remedy that Dr. Hirschfelder has proposed. I want it to be understood that I speak *ex cathedra* to a large extent in this matter, but from a certain knowledge of physiological chemistry and physiology and of chemical processes in connection with organisms and organic substances. It seems to me that there is a possibility that the mode of operation may be recognizable, and it seems to me that it may not be, after all, the striking out of an entirely new line to the extent that it may appear. It all comes, as I apprehend, to the question of the real nature of these organisms that we know as bacteria and their relation to oxygen.

Dr. J. Rosenstirn, of San Francisco: I would have admired the evidence of the truth much more if control experiments with similar liquids also treated in a similar manner with peroxide of hydrogen, that we know is quite a powerful antiseptic in itself, had been made and administered here to-night, showing us that the peroxide of hydrogen itself is entirely unimportant in the causes and effects produced by those experiments of the oxytuberculine on the growth of tubercle bacilli cultures. If we compare the results that Dr. Hirschfelder had the great kindness and industry to show to us to-night, impassionately, with those that have been obtained by the modern methods of the improved tuberculin treatment, such as has been made in the Institute for Infectious Diseases, in Berlin, under the direct guidance of Koch, I must say, and I suppose all those who had the good fortune to observe them will say, that the difference in the results is not a very striking one. We see improvements made and we see failures. While on the whole we always congratulate Dr. Hirschfelder upon the results obtained with this new substance, we cannot but feel that, after all, it is better diluted.

Dr. Hirschfelder, of San Francisco: The matter was not presented to you as a completed work. It will probably take five years of my work and the work of others to solve all the problems connected with it. I simply desire to present to you the evidence that I had thus far collected, to lay it before you and to ask you whether you considered it sufficient to warrant further investigation by me and by yourselves in the same direction. By no means did I think the work was done and that I could present to you a completed thesis. It was only a little part of all of the work that still has to be done. On account of the lateness of the hour I was prevented from making my remarks as fully as I desired, and, therefore, I did not make some points as clear as I should have liked to do. In regard to experiments upon animals, they have been begun. It is very difficult here to get guinea-pigs, and still more difficult to take care of them, with the conveniences at my disposal. Still, something has been done in that direction. Oxytuberculine is only a name. I do not believe that oxytuberculine is a carrier of oxygen. I consider it simply provisional, and oxidized tuberculin simply a tuberculin that has been oxidized with peroxide of hydrogen, and I call it oxytuberculin and give it a name without any special significance. In order to make the experiment perfectly fair, I took plain veal bouillon, the same veal bouillon that I used for my culture experiments, and I treated that with the peroxide of hydrogen in exactly the same manner as I treated cultures of tuberculosis, cultures of streptococci and of staphylococci, of typhoid, of diphtheria, and micrococcus. I oxidized it for 130 hours and the germs grew, and then I destroyed the peroxide of hydrogen and, by the chemical test that I referred to, I proved that it, as well as all the others, contained not a trace of the peroxide of hydrogen; it was all water. The various germs were used to inoculate this oxidized veal bouillon, and the germs grew in that just as they did in the ordinary veal bouillon. With regard to the experiment upon the dog, it was simply made to confirm, for my personal benefit, the observation that was made in the case of Dr. Wyman in tuberculosis of the hand. The tuberculous culture was not put upon the skin, but I dipped a needle into an emulsion of a very virulent culture of tuberculosis, and I vaccinated the skin. Then I covered this carefully with a protecting substance, so that there should be no irritation. After a few days, little elevated red spots appeared, and these gradually increased in size and then broke down and innumerable ulcerations of the skin developed at that point. In the pus of these ulcers a large number of bacilli of tuberculosis was found, and, as I told you, I have the specimens made by Dr. Rixford. With regard to the specific effect, I will have to tell you that all the experiments that I have made, and they have been very numerous, prove the absolute specific effect of these liquids upon the special germ. The guinea-pig is very sensitive to my virulent culture. I have made experiments and had the skin of that guinea-pig nearly cured, when a dog ate up my guinea-pig. I have the skin at home in a bottle, showing the partial healing of the ulceration, but of course that means nothing; the evidence, in order to be of any value, must be absolutely conclusive. The more a statement made is at variance with ordinary experience, the larger the amount of evidence that is necessary before we will accept it, and I desire you, gentlemen, to be skeptical. I do not wish you to accept statements untried. All I desire is, that if my evidence be sufficiently strong to encourage you to investigate the matter, to investigate it for yourselves, skeptically, but fairly.

DR. L. C. LANE, of San Francisco: As we have been occupied here for a long time I will say but little. I have followed the work of Dr. Hirschfelder from the time that he began it, and in the beginning I was very skeptical. I did not believe that he had made a discovery. There was a committee selected by him of members from two medical schools here, of which I was one of the members, to examine into his claims for his discovery. We were partially, but not wholly convinced; we believed in going very slowly, and told him so, though he was fully convinced a year ago that he had made the discovery. Time has gone on and he has done a great deal more work. He has brought here tonight specimens in the test-tubes showing demonstrably, certainly, that this oxytuberculine will control the bacillus and the spores of bacillus tuberculosis. He has worked that out of the test-tube. He has brought here to-night 40 consumptives, some of whom were in the last stage of the disease, and would certainly have been in their graves. They here appear as persons well, and I have carefully listened during the last three hours, and not one of them has coughed.

Special Committee on Increase of Membership in the Society.—In the absence of the Chairman, J. H. PARKINSON, of Sacramento, this report was read by the Secretary. After reviewing the various causes that might be supposed to militate against increase in membership, as well as giving a number of figures bearing on the question, the committee offered the following recommendations: (1) Abolish the present initiation fee. (2) Reduce the annual dues from $5 to $3. (3) Allow any member of a local Society recognized by the State Society to join the State Society at any time other than the annual meeting, on presentation to the Secretary of a certificate signed by the President and Secretary of his local Society. For convenience in bookkeeping such an applicant should pay six months dues. A copy of the transactions for the current year can only be furnished to members so joining, if there be one on hand. At the annual meetings members will be elected in the usual manner and on the usual footing. (4) Any member whose dues have not been paid at the expiration of 30 days next following an annual meeting shall not receive a copy of the transactions. He may, however, retain all other rights and privileges of membership until delinquent for three successive years, when, under the conditions now prescribed, his name must be dropped from the roll of the Society without further action. A list of all delinquent members, showing the number of years for which they are delinquent, shall be included in the Secretary's report, but such list shall not be published in the transactions. (5) Members who have been dropped from the roll for non-payment of dues can join the Society on the same footing as new members—*i. e.*, without penalty. (6) The President-elect shall appoint each year, before the close of the annual meeting, a Judicial Council of five members, who shall hold office for one year, or until their successors have been appointed. The Judicial Council shall organize by electing a Chairman and Secretary. These officers shall, upon requisition, be furnished with stationery and postage. The Council shall hold meetings, confer by letter, etc., and shall, during the year, have charge of all matters in connection with the State Society not already delegated to other officers or committees. This Council shall have full authority to act for and to represent the State Society in all matters, but must not incur any expense other than the above unless so authorized by the Society at a regular or special meeting. (7) For the purpose of harmonizing the organization of the State and local Societies with that of the American Medical Association, each local Society recognized by the State Society shall annually appoint delegates to the State Society, and duly accredit them thereto. It shall be the duty of such delegates, at each annual meeting, to furnish the President-elect, for his guidance, a list of names from each county comprising those members of the State Society resident therein, that are best qualified to serve on the scientific committees, and most likely to contribute papers. (8) That the question of a benevolent feature in connection with membership in the Society needs further and careful consideration. This object could only be met by a voluntary *per capita* assessment, and its successful and permanent operation would be exceedingly doubtful. (9) That the present committee be continued for the purpose of considering the proposed and further changes that may be suggested, and especially such as will affect the manner of conducting the annual meetings of the Society, and of perfecting the details whereby such recommendations as may be adopted can be incorporated in the Constitution and By-Laws. (10) That such recommendations as may be adopted be given immediate effect, where possible, under the Constitution and By-Laws. As the majority of the foregoing recommendations relate to changes in the Constitution, which must lie over for one year, the only amendments possible at this session are those in relation to the initiation fee and annual dues, which provisions are contained in the By-Laws. Your Committee, therefore, offers the following amendment to Section 1, Article VII, of the By-Laws, which should read: Section 1. "The funds of this Society shall be raised by voluntary individual contributions and legacies, by the sale of its publications, by an annual fee of [$3] from each member, which shall be due and payable in advance before adjournment of the annual meeting. Payment of the annual dues shall entitle the paying member to receive one copy of each publication emanating from the Society during that year." [The Society adopted only the recommendation relating to the abolition of the initiation fee, and continued the committee.]

THIRD DAY—MORNING SESSION—THURSDAY, APRIL 23D.

The Practical Value of the Study of the Blood.—A. J. SANDERSON, of St. Helena, read a paper upon this subject. He said: When we consider the important relation which the constituents of the blood bear to the general nutrition as well as to the physiology of the body, we might expect to find in its close examination many things indicative of the real conditions that are present in the various phases of health and disease. It is quite surprising how slowly the study of the pathology or even of the physiology of the blood has advanced in comparison with other branches of medical science. During the past decade much good work has been done in this line. Particularly has this work been interesting and its results profitable when it has been carried on in connection with clinical experience. The facts that are of practical importance concerning the blood can be determined: (1) By the study of the fresh specimen. (2) By ascertaining the percentage of hemoglobin. (3) By estimating the number of white and red corpuscles. (4) By examination of dry and stained specimens. If the future examiners will only become familiar with the elements of normal blood, which is very essential in order to be able to study pathological conditions, much may be learned. In studying the pathology of the blood we have to do with a variety of elements, most of which are peculiar to certain diseases or classes of diseases, and are suggestive of many things relative to the nature of the conditions at hand. The relation that the physiology and pathology of the blood bear to each other is not distinctly defined. The quality and kind of different elements in the blood vary remarkably. The speaker said, for convenience, the study of the pathology of the blood could be classified into those conditions that affected: (1) The red corpuscles. (2) Those that affected leucocytosis. (3) Those affected by parasites. He discussed this classification, entering into the various subdivisions at length, and pointing out the various phases in which this study was particularly helpful in the management of disease.

DR. PHILIP KING BROWN, of San Francisco: It becomes very important in the treatment of certain conditions to know when the danger line is approached, and an examination of the blood in such a disease as, for instance, typhoid fever, illustrates this point. Typhoid is one of the acute processes in which there is no leucocytosis. There are very few diseases in which that occurs. There is even a diminished number of leucocytes in typhoid fever, and the reappearance of health is indicated by the increase in the leucocyte count. Complications in typhoid fever are shown by the appearance of leucocytes; a complication, such as an inflammatory condition of the lungs, pneumonia, produces them. The diagnosis of such diseases as are apt to be mistaken for malaria is another thing that is shown best by blood examination. If you have had dealings with malarial endocarditis, you know that the acute rise of the temperature and its sudden dropping, sometimes quite periodical, might make you feel that you had to deal with a case of malaria. An examination of the blood in this case settles the diagnosis at once. It is not necessary to stain specimens in a case of that kind; simply counting the blood corpuscles would indicate at once whether you had to deal with a septic process or with malaria. The importance of differentiation between primary and secondary anemia is very great. You know that anemia ought to be treated by removal of the cause. The chlorosis of the primary anemia, so defined because we do not know what the cause is, as Dr. Sanderson has told you, needs a different sort of treatment entirely. The absence of leucocytes in one other very general disease will illustrate another important point about diagnosis; that disease is tuberculosis. It is a disease that is particularly interesting to us just now, and the diagnosis of the time that the lung becomes complicated by a secondary infection on the site of the primary tubercular lesion is an important one to make. You know that one of the serious conditions in tuberculosis of the lungs is the formation of a cavity and the secondary infection of that cavity. Pulmonary tuberculosis does not show leucocytes in marked degree until there is an infection of the primary site. It will show a slight leucocytosis when the condition of the patient is bad and the processes extremely active. As Dr. Sanderson said, in all septic processes with a good resistance on the part of the patient there is an evidence of the fight that is going on by a leucocytosis, and that is the one condition in which leucocytes appear in a primary infection with the bacillus of tuberculosis, the evidence that the process is active and that the patient is making an attempt to overcome the influence of that process. When the point is reached where there is a secondary infection the leucocytes increase very rapidly, and if the secondary infection becomes extremely severe the leucocytes disappear, indicating that the patient has no longer the power to resist the process.

Committee on Microscopy and Histology.—The report of this Committee was read by the Chairman, J. C. SPENCER, of San Francisco, the title being "A Brief Summary of the Bacteriological Work in the Laboratory of the San Francisco Board of Health, during the Past Year." He said: The work in the city's bacteriological laboratory, up to the present, has been almost exclusively devoted to differential diagnosis in diseases of the throat and air passages, or, as between diphtheria and pseudo-diphtheritic affections. So far, 190 cases of suspicious throat infection have been examined. Of this number, 50 showed the presence of the Klebs-Loeffler bacillus; in 12 cases, alone, and in 38 cases, combined with the staphylococci, streptococci, or diplococci. The method of procedure is as follows: Previously sterilized swabs kept in test tubes, plugged with cotton, are procured by the physician in attendance, from a drug store or other repository. The swab is removed from the tube long enough only to inoculate it in the throat of the patient. With each swab is a blank which, upon being filled by the physician is returned with the inoculated swab, to the laboratory, on the same day. Here the inoculated swab is rubbed lightly over the slanted surface of the solidified Loeffler's blood serum mixture contained in a test tube. After 10 or 12 hours in the incubator, at 37.5° C., there will usually be a well developed growth of microörganisms on the surface of the serum. It sometimes happens that the growth is scanty and that a diagnosis is not possible. Such a tube is then returned to the incubator for another 8 or 10 hours,

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and the extra period never fails to bring out a sufficient growth from which to make a diagnosis. Portions of the growing colonies are transferred to a cover glass, dried, stained, and mounted in the usual manner, and then examined with an immersion lens. One fact of considerable significance has been observed. Up to the early part of September, 96 cases of diphtheria had occurred and had been properly diagnosed. For the remainder of that month, and for January and February, 1897, suspicious cases were continuously reported for diagnosis, but all proved to be infected with staphylococcus pyogenes aureus. The speaker said it would appear that there had existed in San Francisco, during a certain period in the past winter, a mild epidemic or infection of the tonsils and pharynx, accompanied by the formation of a pseudo-membrane, with elevated temperature and subsequently symptoms of absorption of toxic material. Upon inquiry it was found that the great majority of these cases recovered without receiving antitoxic injections. These cases are certainly infectious. The danger of infection is in no wise to be compared with that of diphtheria, nor are the complications or sequelæ, as a rule, to be feared. The pseudo-membrane has been on the tonsils, fauces, and pharynx, but never in the larynx or trachea. As regards an accurate distinction between the Loeffler bacillus and that of pseudo diphtheria, the rule adopted by Biggs and Prudden, of the New York Health Department, has been followed. "All bacilli which are identical with the virulent Loeffler diphtheria bacillus, morphologically, biologically, and in staining by reagents, should be classed with the diphtheria bacillus, whether they have much, little, or no virulence when tested on guinea-pigs. As regards any conclusive test to distinguish these bacilli, Fraenkel says 'a perfectly trustworthy method of distinguishing between these different bacteria still remains to be found.' "

Dr. Philip King Brown, of San Francisco: I think the importance of such a paper before a meeting of this kind cannot be overestimated, because of the responsibility that we all must feel in regard to the proper diagnosis of diphtheria The opportunities in a community like this are very few for the bacteriological examination of these cases, and the great difficulty that the physicians of San Francisco have met with in securing what is now in working order—this laboratory—indicates what the practitioners in other cities have to go through with. Some time ago Dr. Ernst, of the Bacteriological Department in Boston, gave me this outfit [apparatus exhibited], which is used in Boston for the examination of cases, on condition that I would use it to demonstrate the way they have there of examining the patient.

Dr. Emma S. Merritt, of San Francisco: This whole question of the bacteriological examination of diphtheria is an extremely important one, and I think the city is to be congratulated that we have at last a laboratory. But there are some practical questions in this examination that have brought about a great deal of confusion. I think it is extremely unfortunate that we are unable to distinguish between the true diphtheria bacillus and the pseudo-bacillus. I do not know how Dr. Spencer arrives at the conclusion in some cases that there are true bacillus present and in others pseudo-bacilli. When there are pseudo-bacilli present I would like to know if it is necessary to quarantine the patient? If we are not sure whether they are pseudo bacilli or not, I suppose we have to quarantine them, but where there are cases of diphtheria that we are positive of, we then take cultures of the other members of the family who are perfectly healthy, or other people who have been exposed, and who have these virulent bacilli, and who have no diphtheria, who are perfectly healthy, except that we find these bacilli in the throat. Should such people be quarantined or not? The question of quarantine seems to be a very important one. I have often found bacilli in people who were perfectly healthy, and it has puzzled me very much to know what was the proper course to take in such cases. The Health Office very often in those cases sends the health officers down there, and they label the house "diphtheria," and it seems rather a hardship.

Dr. J. D. Arnold, of San Francisco: Some two or three years ago, when the subject of diphtheria and its prevention and the public means to that end were discussed in New York, Dr. Jacobi, who has always been a strong advocate of the unity of all tonsilitis and of all forms of diphtheria, was asked whether or not he considered every form of tonsilitis a diphtheritic disease, and he unhesitatingly said yes. Such an answer from a man of such enormous experience was a little distressing. When that discussion was reported in the London journals, Dr. Douglas Grant, of London, drew attention to the fact that it had frequently occurred in his practice that where diphtheria attacked three or four members of the same family, he had found that in several of the cases where children of the same family had had tonsilitis, that there were present germs of infection. I do not know whether these observations have received any later confirmation, but I would like to ask Dr. Spencer whether he knows of the relationship the presence of that germ in the mouth, in the tonsil, in the pharynx, may bear to the attack of diphtheria, or if it bears any at all.

Dr. Spencer, of San Francisco: The little apparatus shown you by Dr. Brown is certainly much in advance of the line of progress in the method of simplifying the examinations for diphtheria bacilli. It has been considered a little bit too complicated for general distribution in San Francisco, to put outfits of swabs and tubes of blood serum, etc., in the hands of the members of the profession at large. Without meaning to imply that there is any gross amouut of ignorance in the matter of *technique*, we have considered that it was the more feasible and the better way to avoid confusion and delay to place simply the swabs, and to attend to the rest of the details ourselves. We do not recommend any special restriction as to whether the swab shall be passed lightly or roughly over the surface, leaving that to the member of the profession, but in the majority of cases we have no difficulty in getting at the diagnosis either one way or the other as regards diphtheria. Dr. Merritt said that considerable confusion had been aroused by apparently conflicting diagnoses, clinical and bacteriological. It is true that we take the stand of labeling all cases diphtheria which bear any resemblance to a diphtheritic affection, and when we find a bacillus which takes the aniline stain in a somewhat fragmentary form, rather sharper at the poles, with little unstained spaces between, we consider that a case of genuine diphtheria. In the other members of a family, where one child is suffering from diphtheria, there may be no apparent clinical symptoms of diphtheria. It is a matter of record, that Klebs-Loeffler bacilli are found in the throats of healthy individuals, and that they persist in the throats of individuals who have suffered from an infection for quite a long period, sometimes as long as four, five or six weeks. When we find short rod forms, as we do almost constantly from the infected swabs, we do not hesitate to class these as diphtheria, not because they are regarded as genuine diphtheria bacilli, but as a matter of safety. We have, perhaps, been a little bit careless in the matter of distinguishing to the profession what we consider to be genuine diphtheria, and what we consider to be pseudo-diphtheria, and in order to set the practitioner straight in this matter we have adopted the plan of mentioning the facts specifically; that we do not regard the examination as satisfactory or as typical, that we regard the case as a pseudo-case, and await developments. The pseudo cases are certainly not as infectious as the genuine diphtheria cases, and although our method at the health office has been to placard the house, we have in many instances sought to modify the moral effect on this house by instructing the practitioner, the Sanitary Inspector and the family, that only a modified quarantine would be insisted upon, and that prohibition of school attendance would not be enforced upon other children of the same family, it being simply insisted upon, that the infected individual should be kept strictly secluded for at least a week, when he would be released from quarantine. We certainly do not stand before the members of the profession in this branch of the Health Department, as being in perfect running order as yet. The office of Bacteriologist has been in existence a little over a year. The city has had its own laboratory less than six months, and if we have been criticized on the score of creating uncertainty in the minds of the profession, and perhaps in the lay mind, we will simply ask for a little extension of time until we may have so perfected the working methods of the department, that confusion will no longer exist. As regards Dr. Arnold's query, I am not in a position to answer it definitely. The modified relation of the previous existence of the leptothrix upon subsequent diphtheritic exposure, has been more or less touched upon, but I believe that no definite conclusion has been arrived at. The matter is still *sub judice*.

Catheterization of the Ureters.—M. KROTOSZYNER, of San Francisco, read a paper upon this subject in which he reviewed the advances that had been made in this operation, not only in the female but in the male. This had been largely rendered possible by improved apparatus and perfected *technique.* The speaker pointed out the diagnostic importance of the measure and the grave dangers, especially in operative renal surgery, that could be avoided by its employment.

Committee on Dermatology and Genito-Urinary Diseases.—GRANVILLE MACGOWAN, of Los Angeles, the Chairman of the Committee, presented its report, the subject being, "Treatment of Stricture of the Urethra by Fort's Method of Electrolysis, with a New Instrument for its Application." The speaker briefly discussed the pathology of this affection and then described the method of linear electrolysis that has been recommended by Fort. This the author had found most successful. Its performance was simplified and rendered more exact by the use of the instrument which he had described. Other operations for stricture were mentioned and the author admitted that Fort's method was not a universal one. He reported a number of cases upon which he had operated in this manner; in all of these the results had been highly successful, and these results have been attained in a very short time.

DR. L. BAZET, of San Francisco: Before attempting to treat urethral stricture by any method, the lesion ought to be determined. Any treatment that will be directed against the lesion will be an ideal one. Have we got this in the method of electrolysis? When the treatment of urethral strictures by cauterization was abandoned because of the production of cicatricial tissue, dilation and internal urethrotomy, took the important position they now occupy. The idea of removing the cicatricial tissue was what caused electrolysis to be admitted into surgical practice.

DR. G. GROSS, of San Francisco: Dr. MacGowan did not state the number of milliamperes he is using. This is important, if we use only a few milliamperes, four or five, or perhaps up to ten, the operation is a simple dilatation, there is no local effect, while if you get above 30 or 40, it is cauterization, and judging from the rapidity with which Dr. MacGowan succeeds, he must have used a very high number of milliamperes, and therefore he must have produced cauterization and a scar as a consequence. This question has been settled experimentally. The little experience I have had with it has not been satisfactory. The general impression has been that the results have been rather unfavorable and that it is inferior to section by the knife. I have been astonished at the uniform success which Dr. MacGowan has encountered. It may be that his cases are too recent; that he has not had a chance to follow them long enough.

DR. MACGOWAN, of Los Angeles: If the doctor would have taken the trouble to follow, with a spirit of fairness and scientific inquiry, the manner in which the cases were reported, and also my remarks about the other kinds of operations, he would have seen very easily that I did not recommend the method of electrolysis of Fort, as one for strictures in general. I have at my command probably more material of this character than any man on the Pacific coast. I have perfect command of my material, and I can follow my patients, and I have a number of other persons to follow them for me. In the hospital, a strict and absolute history of these cases is kept. The method is advanced simply for the purpose of adding another means to the armament of the surgeon for the treatment of stricture of the urethra. I cut a great many cases of stricture of the urethra. I cut them in all ways, externally and internally. I use my Maissoneuve freely. I do not get any better results from the use of it, than I have gotten from this instrument. It is certainly less dangerous. The milliamperage used is not sufficient to produce an exterior effect. When you use a milliamperage of 130 or 140, or more, your knife glows. I did not take accurately the number of milliamperes required for the production of this particular result with this particular instrument. I certainly know it was not 3 or 5; I certainly know it was not 30 or 40. I stated that I performed a *post-mortem* examination on one of them; I have the specimen in my collection. I saw exactly the kind of scar it produced there, and how far it produced it. The autopsy was made within two hours after the man died, and the material was perfectly fresh. I would be far from advocating the method of Fort as a universal one for treating strictures. In those cases of old strictures that are hard, resilient, and dense, through which you can only, upon most careful manipulation, pass a filiform, and when seated beneath the arch of the pubes, it is dangerous to cut, and some other operation is necessary, which, though it may not cure, produces a channel through which an individual can urinate with a fair degree of drainage for his urinary organs. That is all I claim for it. I have now been attending to this specialty for a number of years. I have operated a good many times with my Maissoneuve, and I have seen when these operations were conducted with as perfect asepsis as it is possible for me to get, when the individuals were wealthy and able to take care of themselves and had nurses to take care of them. I have seen these strictures recontract, and so has Dr. Bazet, and so has every surgeon who operates much in this portion of the urethra. I do not want to be understood as advocating this method of electrolysis, or any other method of electrolysis in general, for the treatment of strictures. I simply say that my experiments during the past have been such as to lead me to say that in chosen cases this method has a very distinct value.

A Case of Extensive Leucoplasia of the Mouth Eventuating in Epithelioma of the Cheek and Tongue.—DR. D. W. MONTGOMERY, of San Francisco, reported this case. He said: The patient, a man 46 years of age, had smoked tobacco excessively from earliest boyhood. Twenty-five years ago he noticed a roughness in both cheek pouches, the surface of which he could scrape away with pipe or lead pencil. There was also an uneven patch on the back of the tongue in which he felt a burning sensation when he smoked. The patient tried to reduce the amount of tobacco used, and ten years ago stopped smoking cigarettes, one year ago ceased using the pipe, and about two months ago gave up cigars. There was extensive leucoplasia of the tongue and inner surface of lips and cheeks; a well developed epithelioma in the right cheek pouch, and a commencing epithelioma on the dorsum of tongue. The lymphatic glands beneath the jaw on the right side were enlarged. The cancers were removed by Dr. H. M. Sherman, March 5th, 1897. It is of interest that there is shown every stage from simple leucoplasia to well developed epithelioma in the same case. It is unusual to find two cancers at the same time in the mouth. There was absolutely no history of syphilis.

A Case of Perineal Urethrectomy.—L. BAZET, of San Francisco, reported this case: The patient, a Portuguese sailor, aged 46, came under his care in December, 1896. Twenty years previously he had had a gonorrhea, which had since constantly been under treatment. In 1895 he was seized with acute retention of urine, which yielded to opiates and warm baths, but from that time on he went from bad to worse, the obstruction steadily increasing, so that he was compelled to abandon his work. Examination showed an obstruction 4½ inches from the meatus, impermeable to even a filiform bougie. A similar attempt, under chloroform, was equally unsuccessful. An external urethrotomy was performed, the orifice of the structure was found, but it was still impermeable. Suprapubic cystotomy was next resorted to, and two quarts of urine were evacuated. A sound was then passed through the vesical neck and forward to the structure, and cut down upon over the posterior aspect. No filiform bougie could be passed. The strictured portion of the urethra measured one inch. Division of this failed to reveal any healthy mucous membrane and it was therefore excised *en masse.* The ends of the urethra were next united, and when finished, the lumen of the canal appeared to be hermetically sealed. A Pezzer sound, which had been passed prior to the suturing, was left in place for 22 days, after which, the patient was catheterized three times in 24 hours. The patient, on leaving the hospital, was unable to dispense entirely with the catheter. The urethra admitted a No. 8 sound, and the bladder retained 200 gm. of urine. At the present time a No. 25 French sound passes easily. He can hold 14 ounces of urine in his bladder and the stream is projected with considerable force.

AFTERNOON SESSION.

A Study of the Blood in 73 Cases of Tubercular Bone Disease in Children, with Reference to Prognosis and Treatment.—Despite the fact that the authorities on blood examination state that pure tubercular processes, no matter where situated, show only slight alterations in the blood, much can be determined from a careful study of these alterations in their relation to certain clinical symptoms. To secure uniformity in conditions the blood examinations should be made just before a meal, when the digestive leucocytosis is over. The general results of the examinations are: (1) There is no decrease in the number of red blood corpuscles, except in secondary anemia of the second or third stage, which comes (*a*) in long standing and extensive cases; (*b*) in very young children; (*c*) in septic complications. (2) Hemoglobin is decreased in all cases in relation to the same factors which influence the red blood corpuscles. (3) A return to health is indicated by the tendency of both conditions to return to the normal. (4) Abscess formation is not necessarily accompanied by leucocytosis, but a slowly developing leucocytosis means an activity of the tubercular process, and a rapidly developing leucocytosis means a secondary infection of the abscess with pyogenic organisms. A tubercular abscess may be formed and reach considerable size and be absorbed without a leucocytosis developing. (5) In septic infection of wounds from operations on cases of tubercular bone disease the leucocytosis is marked at first, and diminishes as the resistance of the child increases or decreases. If the decrease is accompanied by an increase in the percentage of hemoglobin and in the number of red corpuscles the prognosis is good. If it be accompanied by increased anemia it is a sign of a diminishing vitality of the child. (6) The leucocyte count bears no relation to the temperature. The speaker said the 73 cases studied were from the clinic of Dr. H. M. Sherman at the Childrens' Hospital and from his cases in private practice. This study confirms largely the work of Dane, of Boston, on the same subject.

Vis Medicatrix Naturæ.—WM. J. G. DAWSON, of St. Helena, read a paper upon this subject. He said: There are two classes of cases occasionally presented to the care and consideration of the physician that cause him to fully realize how imperfect and helpless he is. (1) Wherein medical science has always been of little avail, and where the patient invariably dies of the disease. (2) Where Nature steps in and takes the case, as it were, out of his hands, performing a perfect cure. The first class may be represented by a case of tubercular meningitis, in which the physician can do little but watch his patient through the different stages of the disease, and from the first—if his diagnosis be correct—is compelled to give an unfavorable prognosis. The second class is represented by nature's perfect method of care in empyema of children; also, in her cure of tubercle at the apex of the lung in adults. During my hospital life in New York, it was not uncommon to find on *post-mortem* examination an old cicatrix at the apex of the lung, showing nature's cure of consumption, the patient having died of some other disease. This cure cannot be surpassed, or perhaps equalled, by any of our modern plans of treatment. Four years ago I reported to this Society a case of empyema in a child 6 years of age. I stated that I had during the months of January and February, 1893, aspirated the chest seven times, withdrawing in all about 73 ounces of pus. On February 7, 1893, the day following the last aspiration, the child was seized with a violent fit of coughing, and expectorated freely of pus, showing conclusively that a spontaneous perforation into the bronchi had taken place, followed by rapid recovery. One year later I examined the child, and found a perfectly normal condition of things. There was no external scar, no collapse of the chest cavity, and to-day the child is a finely formed, healthy specimen of boyhood. Unfortunately nature, unaided, does not always produce such perfect results, but where she does in the manner described, the cure is more perfect and complete than if brought about by any known surgical means. The speaker said: One of the objects of this short paper is to promote discussion on the best known and most successful methods of imitating or aiding nature in the cure of diseases of the chest, especially in children.

Evisceration versus Excision of the Eyeball in Panophthalmitis.—K. PISCHL, of San Francisco, read a paper upon this subject. After describing the symptoms of panophthalmitis, he said: Since Von Graefe, in 1863, had lost two cases from meningitis, after excision of a panophthalmic eye, he concluded and convinced the ophthalmic surgeons that enucleation is dangerous and contraindicated in this condition. Since then the treatment has been very conservative, and has been confined to ameliorating the patient's suffering. Narcotics were used against the pain, and hot poultices applied to hasten the process. In case perforation appeared unavoidable, a free incision of the sclera in its anterior division was made. The tension was then diminished, and evacuation of the suppurating contents of the eye accelerated. When the eye at last became shrunken, it usually remained quiescent and admitted of an artificial eye being worn over it. Unfortunately quite a number of cases have been observed in which meningitis and death have resulted from panophthalmitis, therefore it was advisable to look for some better treatment which would remove the infectious matter from the eyeball as early as possible, because infection may occur any time from an eye which is suppurating, and the longer the pus is shut up in the eye the greater will be the risk. The duration of treatment is also greatly diminished. There are two operative procedures for that purpose: (1) Excision of the eyeball. (2) Evisceration. Excision is still considered contraindicated by a great number of surgeons. Over 50 cases of death from meningitis after enucleation have already been published, and among these excision during panophthalmitis furnished the majority. In enucleation of a panophthalmic eye the danger of infection of the wound is very great. Strict antisepsis may diminish, but will never entirely overcome the danger of infecting the wound, because we come too late with our antiseptics, and the wound itself contains so many sinuses and folds that strict disinfection is impossible. From the wound the infection can be carried on through the opened blood-vessels and lymphatic spaces to the meninges. The early removal of the infectious contents, which is so advisable, can be accomplished without danger of further infection by evisceration or exenteration of the eyeball. In this operation we have a clean wound, surrounded by the dense sclera which can be disinfected in the most stringent manner. As all the contents of the eyeball can be entirely removed, we need not have any fear of sympathetic inflammation. As this operation preserves the sclera with its muscular attachment, we have a large movable stump, which can bear an artificial eye to much greater advantage. The too apparent loss of an eye is always a great hindrance to any one applying for a position in any capacity. For this reason evisceration is preferable in such extensive injuries where the eye cannot be preserved. In many cases of panophthalmitis the patients cannot be induced to allow enucleation, but will consent to evisceration. The operation is not difficult. According to Alfred von Graefe's method, it is performed as follows: The cornea, together with an adjacent zone of the sclera, is removed by first inserting the sclera near the limbus with a knife, and then detaching it by a circular cut with the scissors. The contents of the eyeball are scooped cleanly out with a sharp spoon, so that the inner surface of the sclera lies exposed. Lastly, the opening is closed again by means of sutures passing through the conjunctiva and the cut edges of the sclera. After the operation, some reaction may set in, the lids and conjunctiva may be red and swollen, but this will pass off in a few days. The blood clot which fills the sclera becomes vascularized, and the eye slowly shrinks to the size of a phthisic eye. Mules introduces into the empty sclera hollow glass balls; Lang, balls of celluloid.

Committee on Ophthalmology.—A. H. VOORHEIS, of San Francisco, the Chairman of this Committee, instead of a formal report or paper; presented two cases of immature cataract which he had operated upon, and reported the procedure adopted in each instance.

DR. A. BARKAN, of San Francisco: We all come across cases which mature so slowly that they become a great torment to the patient and to the physician. In such very exceptional cases, the operation on unripe cataract is admissible and will, in a given number of cases, be successful. In removing an immature cataract, where part of the unripe cataract necessarily remains behind and must be removed, either by scooping the anterior chamber out or by irrigating it, no matter how clean your irrigation tube may be, no matter how neutral the solution which you use, no matter how aseptically you douche the interior of the eye, I think we will all feel that every once in a while harm must arise to such eyes from procedures of that sort. My experience, at any rate, has been that in the general run of cases of cataract, once the cataract is extracted, I refrain from entering the eye with scoop or hook or irrigation tube or any instrument whatever. I much prefer leaving some unripe or tenacious cortical matter behind, trusting to a gradual absorption and to a proper healing of the eye, and I have no reason to be sorry that I have proceeded in this very conservative way. Consequently I would say that, as a very exceptional thing, the removal of an unripe cataract is permissible. It has been done for a very long time by men such as Panas, in Paris, Lynnicott, of Pittsburgh, and others, and it is a very fine thing to succeed, and we surely must congratulate Dr. Voorheis on the good success he has achieved in these cases; but as a method, I certainly would not approve it. I make the point in instructing young men going out in practice, that they should not touch any case of cataract unless they feel specially qualified to do so. If an operation must be done, they should not operate unless the cataract is fully ripe, then pick it out of the eye as a whole, just as you would pick fruit from the ripened stem. Dr. Pischl's paper is a very important one, and puts the matter of operating in cases of panophthalmitis clearly before us. I will refer to one point which he makes, in correction, viz: that in cases of panophthalmia, eyes undergoing atrophy, after rupture and evacuation of pus, are not considered, as a rule, likely to produce sympathetic ophthalmia. Fuchs, and Swanzy in his last edition, are very emphatic upon this point. It has been my ill fortune to have had a case within the last three months in which this statement made by such eminent authorities, is shown to be incorrect. I deem it my duty to very briefly report the case, as it opens up a hitherto unsuspected vista in panophthalmitis, the danger of sympathetic ophthalmia to the other eye. A young girl. 18 years of age, who had been since her childhood under my care, on and off, for scrofulous ulceration of the cornea, having enjoyed, for several years past, perfect freedom from any recurrence, having grown into maturity fully, and having some sight with the eye, which had recovered vision, so that she could see the hands of a clock across the room, was suddenly attacked with a most virulent type of panophthalmia, from no cause that we could ascertain. She had been well; there was no traumatism, no source of purulent secretion in any part of her body or around her. I was called at Christmas time, and found the eye in typical ophthalmia, pressed forward, and such chemosis that the eye seemed to be surrounded by a dark red conjunctival wall, the lids were tremendously swollen, and the girl was in a condition of high fever and suffered a great amount of pain. I ordered her to go to the Lane Hospital, and the very next morning, under ether, and with antiseptic precautions, I made an extensive opening of the interior of the eye and evacuated a lot of greenish-yellow pus. The swelling subsided, the eye commenced to shrink a week after the incision had been made, and for three weeks she did so well that I mentioned to my assistant that within a couple of weeks I thought it would be safe to remove the stump, as I always do; for I find that if you remove a stump cleanly, taking care to thoroughly cleanse and dissect off the conjunctiva and to detach every tendon, you will get a perfect conjunctival bag, which is drawn to one side and the other by the ocular muscles, and you will have an excellent fit for the glass eye. I maintain this against any statement to the contrary, and also against that of Dr. Pischl, that stumps within the eye and glass eyes over them give better ocular effects. Six years ago I spent two months with Professor Graefe, of Halle, in the very place where the method of evisceration has found its origin. It so happened that Miller, an artist in the manufacture of artificial eyes (who had also visited San Francisco a few years ago, and who gave a clinic at the Cooper College and showed us the process of manufacture); it so happened that this man came to Halle to fit the former patients of Professor Schwartze, with artificial eyes. I have never seen a more cross-eyed lot of people. I told him our people in the United States would not put up with such fits; they want the best teeth and the best eyes, and we could not do the operation if after we had such convergence of vision. To return to the girl: After four weeks' time, the child being nearly ready to have the stump removed, she came to my office one afternoon—I only saw her every three or four days—and there were the first symptoms of iridocycletis in the healthy eye. A severe plastic iridocyletis has since been going on, the pupil is nearly closed, no matter how much atropine had been used. The eye is now in condition that the patient can see fingers five feet distant. She may or may not keep that amount of vision. The virulence of the affection is rather passing away, and there may be a chance later on, by iridectomy, to improve the vision. But the fact remains that, having proceeded in a perfectly surgical way, and I think in the way in which every one of us would have proceeded in the removal of the pus, sympathetic ophthalmitis followed. Personally, I do not approve of evisceration in cases of panophthalmitis, but rather opening the interior of the eye and evacuating the pus. Nevertheless, the infectious germs may extend from one eye to the other eye, and sympathetic ophthalmia be produced.

DR. PISCHL, of San Francisco: Regarding the operation for immature cataract, of which Dr. Voorheis has shown us two cases, and on the result of which I congratulate Dr. Voorheis, I would like to say that to operate upon immature cataracts without iridectomy, appears to me to be rather dangerous. We cannot easily remove the rest of the cataract; therefore, very frequently, as Dr. Voorheis himself remarked, we get occlusion of the pupil. Besides, there is always the danger of prolapse of the iris. I would like to mention a method which is quite frequently followed of making a preparatory iridectomy, and making the extraction about four or six weeks afterwards.

DR. VOORHEIS, of San Francisco: This matter of immature cataract being one which falls under my observation, and wanting experience and light from some other source besides that of my own experience upon it, I should have been delighted to have found some one who could have given me some sort of encouragement. I said at the beginning of my remarks that the hand of conservatism would ever be raised against the removal of immature cataracts as a general operation, and that we could not think of it for a moment for obvious reasons. Dr. Pischl's paper was most interesting, and his points were well taken. One word in regard to evisceration, which I find does not meet altogether with the approval of my friend Dr. Barkan. I am sadly disappointed in two cases which promised to be here from the country illustrating this very point of reaction. I have now operated 13 times. Twelve out of the 13 are all that I could possibly wish. They are wearing their artificial eyes with perfect comfort. There is no disturbance whatever; they are as natural as it is possible for an artificial eye to be made, and the movement is nearly normal. The only case which was not satisfactory left here, after 20 or 30 days, and went home to Virginia City. After being there some 6 or 8 months, she was taken with some low sort of fever, and, as a result of incessant vomiting and retching which lasted for days, a perforation in the sclera resulted, so that a portion of the glass ball was exposed. She came to see me again, and as she lived so far away, and fearing some septic matter might go through this perforation and into the sac and create a disturbance of the same nature, I thought it best to remove the glass ball. It was done, leaving the sclera to collapse; an atrophic condition has taken place, and upon that she wore the artificial eye with comfort and with a very good appearance. This operation should not be made upon any individual who has to go to work within six weeks or two months. The reaction in every one of these 13 cases that I operated on was considerable, and lasted from six to twelve days. I removed the sutures in none of the cases in less than twelve days. To laboring people who have to go to work we must say, "I would like to make a glass-ball operation, so to speak, but it will delay you and keep you from work from six weeks to two months, and therefore I cannot recommend it." After an experience of 13 cases, I feel that we owe that explanation to the patient.

Committee on Laryngology, Rhinology, and Otology.—J. D. ARNOLD, of San Francisco, the Chairman of the Committee, read its report, the subject being "Some Unusual Forms of Laryngeal Paralysis." The speaker reported the following cases: *Case 1.*—Miss S——, æt. 19, ever since an attack of diphtheria, which happened three years previously, had suffered from loss of voice. Faradism, galvanism, and strychnine were used without result. When examined by the speaker, the throat was normal in development and its tissues healthy. The respiratory movements were perfect, but on attempted phonation, the left cord remained nearly immovable, while the right advanced slightly beyond the median line of the glottis. At a subsequent examination it was discovered that upon touching the inter-arytenoid space with the tip of a sound, slight isolated cough acts were produced, and then, as is the case also in the normal larynx, the cords met for an instant in the middle of the glottis, before the expiratory effort took place; and further, that the cough had tone. Later, with very delicate manipulations of the arytenoids with the sound, the lame cord could be excited to action without producing the cough. Systematic treatment was begun. Four times a week, a weak Faradic current was used, one electrode on the neck over the left wing of the thyroid cartilage and the other into the left pyriform sinus. After three weeks she could give tone to the vocable A, with broad inflection. Other vocal sounds were gradually added and two months after her first visit she could read 300 words in a clear, distinct voice. *Case 2.*—Mrs. I——, æt. 46. After five months' treatment by a local laryngologist, for loss of voice and pain, she had not improved, and her attendant finally diagnosed a malignant growth of the larynx. Examination by the speaker showed a dusky red color of parts and a tumor which seemed to spring from the ventricle. There was no loss of continuity of the mucous membrane anywhere. When the patient attempted to phonate, the tumor floated on the ascending column of air, but the whole left side of the larynx remained immovable. The swelling proved to be a prolapsed ventricle. It could be easily returned to place by a sound, but the slightest cough or effort to produce voice, prolapsed it. Some suspicion of specific trouble led him to adopt an anti-specific treatment. The patient was seen every other day, and the ventricle replaced each time. The use of the voice was forbidden. In 15 or 16 days, when the mercury began to show its physiological effects, by tainted breath and tender gums, the appearance of the larynx notably changed. The parts regained their normal color. The prolapse of the ventricle took place less readily. Further local treatment with iodine and constitutional treatment with careful and gradual use of voice, resulted, after six months, in a cure.

Microbes in Suppuration of the Middle Far.—GEORGE W. MERRITT, of San Francisco, read a paper upon this subject. He said purulent inflammation of the middle ear is one of the most important of middle ear diseases, not only on account of the deterioration of the ear, but also because of those dangerous complications sometimes produced by the spreading of these processes to the cranial cavity. The reason why the disease was so persistent was owing to its attacking the tympanic cavity, the part which is hardest to get at. The outlet for discharges is so small that there was little chance for their escape. Syringing did not reach them. Many of the cases did not consult the aurist until the disease had run a long course and was in a decidedly chronic state. As no suppuration occurs without the presence of microbes the question arises, how do the microbes reach the middle ear when the drum is intact? There are two possible avenues through which they may reach the cavity. First, through the circulation and stases occurring in the capillaries of the mucous membrane of the middle ear, producing an abscess, which ruptures into the cavity. The speaker believed that this method, though quite rare, was possible. The second, and by far the most common, was due to direct extension of the disease from the throat to the middle ear by way of the eustachian tube. Miller has isolated 22 different species of microbes in the mouth, and Black has found in the saliva of ten mouths the presence of staphylococcus pyogenes aureus seven times, s. p. albus four times and the streptococcus three times. These microbes are harmless until some leison of the mucous membrane occurs, when they can at once begin their work. In middle ear suppuration the severest complication, namely, intra-cranial inflammation is, according to Moos, produced by the streptococcus and the diplococcus of pneumonia. This latter is found mostly in the otitis following pneumonia, or in that due to "catching cold," the streptococcus occurring more frequently in cases of traumatism, and in those due to the presence of foreign bodies. When suppuration of the middle ear has been cured the mucous membrane for a long time is very sensitive and liable to cause earache on the slightest change of weather. Unless great care is exercised there is likely to be a recurrence of the suppuration, as the microbes may remain dormant for a long time. The speaker exhibited a number of cultures that had been made at the laboratory of the San Francisco Polyclinic from cases of chronic middle ear suppuration under his charge. These showed the various microbes concerned in the suppuration process. In some cases mixed infection was present.

A Case of Sinus Thrombosis—Operation—Recovery.—J. D. ARNOLD, of San Francisco, reported this case. He said: A girl of 13 years had developed purulent otitis during the course of measles occurring at the age of five. The otorrhea continued with many remissions for three years. In 1890 the mastoid became implicated, and a Wilde incision was made by another aurist, which liberated a subperiosteal abscess. The symptoms were somewhat relieved until 1896, when the mastoid trouble again appeared. Very grave cerebral symptoms followed, and it became apparent that the sinuses were affected. The more prominent symptoms were frequently recurring chill, followed by a temperature of 104° F., vomiting, and a tendency to coma; optic neuritis, dilated pupil, convergent strabismus and ptosis. By an operation the transverse sinus was laid bare after Schwartze's method. The wall was discolored and presented itself as a hard rigid cord, entirely without pulsation. The sinus was incised longitudinally and a firm fibrous clot 2½ inches long removed. No bleeding followed. The sinus was irrigated with sublimate solution, and the vein and mastoid cavity were plugged with borated gauze. The recovery was slow and tedious, but otherwise uneventful.

DR. GEO. H. POWERS, of San Francisco: The suggestion that Dr. Arnold's first case gives of the advantage of mechanical irritation of the vocal cords in aphonia, is something that I do not remember to have seen in print. I think that it is a very valuable proposition. It has seemed to me in certain cases in which I have myself used solutions of nitrate of silver in the larynx, in cases of partial aphonia, and in cases combined with inflammation of the parts, that the effect of the nitrate of silver was, while very beneficial, not entirely due to its entity as nitrate of silver, but to the fact that it was an irritant. I have often had this brought to my mind in watching these cases that the mechanical irritation was a matter of great importance. The use of electricity I have never found to be beneficial, and I have always doubted whether it could be used with benefit, but there may undoubtedly be exceptions to that proposition. Dr. Merritt's paper, it seems to me, is very valuable in bringing up the question of bacterial influence in these long-continued troubles of the ear, which we knew so little about until bacteria were presented to our knowledge. Before we had bacteria we had inflammations of the ear that must not be stopped; if they were checked the patient was going to die of inflammation of the brain, and so far back as the time when I was young, I remember very often to have heard physicians, as well as patients and nurses and old friends, say that an inflammation of the ear must not be checked; it must be allowed to run its course. We know now why the inflammation does run the kind of course it takes; we know why the inflammation recurs after it is once stopped, and such studies as those Dr. Merritt has presented give the rationale of the movement. The last case of Dr. Arnold's was to me a very interesting one, as being something that is entirely beyond my experience. I have never seen one of these sinus operations made. I had myself the misfortune, if it was such, to bore into the sinus. The operation was simply as a last resort; the patient was practically dead when the operation was made and the sinus was opened. What the result would have been if the patient had been really a living body, and had a chance to recover, I do not know; but the fact that a large clot was removed in this case, and that the patient recovered, seems to me something that we may all know and remember with advantage; for such cases may occur again.

Executive Committee.—This Committee, through its Chairman, W. LE MOYNE WILLS, of Los Angeles, presented the following report on the various matters submitted to it: *Recommendations of President's Address:* (1) San Francisco Board of Health: Recommends the hearty endorsement by the Society of the Board's action looking to the prevention of the adulteration of foods and food products. (2) Preference of members in practice: Recommends that the Secretary of the Society shall keep a record showing the direction of interest and preference in practice, of individual members, so as to assist the President in his appointments of the various committees. (3) Amendment of by-laws: Recommends that the amendment by Dr. E. K. Smith be referred to the special Committee on Increase of Membership. (4) Contract practice: That the action of the Santa Clara County Medical Society, in reference to contract practice, be indorsed and adopted by this Society, in accordance with the recommendation of the Executive Committee of 1896. *Bernalillo County Medical Society;* Recommends the adoption of the resolutions forwarded by the Bernalillo County Medical Society, of New Mexico, in reference to the publication of climatological and meteorological reports throughout the United States, by the government. *National Medical Legislation:* Recommends the adoption by this Society of the resolutions in reference to national medical legislation, presented by R. Beverley Cole and W. LeMoyne Wills. *Reduction in Fees of Medical Examiners for Life Insurance:* The Committee notes with pleasure that the protest of the various county societies of this and of other States has had the effect of causing life insurance companies to return to their former fee bill. This shows the beneficial effect of concerted action by the medical professson. *Increase in Membership:* Recommends that all members use their individual efforts to enlarge the membership of the Society, and that papers for the annual meeting be made of as practical a nature as possible, or less theoretical, and such as to bring out a thorough discussion of each topic; also that, in future, fewer papers be placed on the programme.

New Members.—At the different sessions the following were duly elected members of the Society:

W. M. S. Beede,
E. D. Brinkerhoff,
L. L. Dorr,
George N. Drysdale,
G. W. Dwinnell,
G. L. Eaton,
George H. Evans,
F. Fehleisen,
J. C. Fisher,
Campbell Ford,
John Gallwey,
G. Gross,
O. D. Hamlin,
S. J. Hunkin,
P. M. Jones,
F. C. Keck,
E. E. Kelly,
H. B. A. Kugeler,
J. Maher,
H. P. Mathewson,
W. H. Maxon,
J. F. McCone,
A. B. McKee,
J. J. Medros,
Mary E. Morrison,
D. E. Osborne,
R. W. Payne,
G. B. Richmond,
J. C. Stinson,
Kate P. Van Orden.

The following were reinstated:

A. Fine,
D. A. Hodghead.

The following were elected honorary members:

Lieutenant-Colonel Chas. R. Grenleaf, U. S. A.; Lieutenant-Colonel J. V. D. Middleton, U. S. A.

MEDICAL NEWS.

Official List of Changes in the Stations and Duties of Officers serving in the Medical Department of the U. S. Army (Division of the Pacific) from March 20, 1897, to April 20, 1897.

Leave of absence for one month, with permission to apply to the Adjutant General for an extension of one month, is granted Major Van R. Hoff, Surgeon. S. O. 62, Dept. of the Columbia, April 14, 1897.

Leave of absence for six months, to take effect on or about May 18, 1897, is granted Captain Edward R. Morris, Assistant Surgeon, Fort Spokane, Washington.

Captain Nathan S. Jarvis, Assistant Surgeon, is relieved from duty at Willet's Point, N. Y., to take effect on the expiration of his present leave of absence, and ordered to Fort Huachuca, Arizona, instead of Fort Clark, Texas, for duty.

Official List of Changes of Stations and Duties of Medical Officers of the U. S. Marine Hospital Service (District of the Pacific) from March 20 to April 20, 1897.

Past Assistant Surgeon S. D. Brooks, to proceed from Port Townsend, Wash., to principal ports of Japan and China on special duty.

Past Assistant Surgeon W. G. Stimpson, to assume charge of Marine Hospital Port Townsend, Wash., in addition to quarantine station, during the absence of Passed Assistant Surgeon S. D. Brooks.

ITEMS.

Dr. B. S. Nourse has removed from 1819 K street, Sacramento, to Franklin, Cal.

Dr. Sidney E. D. Pinniger has removed from Reno to Cortez, Eureka county, Nevada.